THURSDAY, JUNE 17, 1875

CROLL'S "CLIMATE AND TIME"

Climate and Time in their Geological Relations; a theory of Secular Changes of the Earth's Climate. By James Croll, of H.M. Geological Survey of Scotland. (London: Daldy, Isbister, and Co., 1875.)

MR. CROLL is well known as an original thinker of considerable power, who has turned his attention to the physics of geology, and has produced a series of remarkable papers on questions of the highest interest in that subject. His views are opposed in many respects to those accepted by other influential thinkers, and have given rise to a considerable amount of controversy. Hitherto they have been scattered in papers to various periodicals, and it has been difficult to obtain a consecutive view of them. The work which is now issued, while not an actual reprint of previous papers, is a complete exposition of their contents, or at least of that part of their contents that Mr. Croll is prepared to stand by, some of the arguments that occur in his papers being omitted in his book. We are now therefore able to judge fairly what truth there is in Mr. Croll's ideas, and to compare them with those of his opponents. Even were all his ideas untenable, we should still have to thank him for his vigorous discussion of these interesting questions, but there can be no doubt that in many instances he proves his point.

Mr. Croll does not possess the happy faculty which some authors have of carrying his readers with him: on the contrary, his style is so controversial, that to agree with him is to have the feeling of being vanquished, and the reader is throughout set on his metal to find out some flaw in the argument. This, as in most cases of controversy, it would not be difficult to do; but we must confine ourselves to the discussion of his main results.

One peculiarity of Mr. Croll's arguments must here be noticed. After having assumed certain figures and arrived by their means at definite results, he proceeds to show that these figures are unreliable, and then to state that their unreliableness will not affect his results; or else, in order to bring his results more into accordance with received opinions and probable facts, he generously halves them or diminishes them still more, apparently unaware that had his arguments been correct and his first results the true ones, he would have proved too much and refuted himself. Examples of this peculiarity will be seen in the sequel.

The first question discussed is the heating influence of the Gulf Stream. To estimate this Mr. Croll uses the method of heat units, and prides himself on doing so. The method is an undeniable one, and is perhaps the only one by which the influence of the high specific heat of water can be made manifest. Mr. Croll compares the number of foot-pounds conveyed by the Gulf Stream into temperate regions with the number due to the heat of the sun shining directly on those regions. The relative value of these depends on the absolute value of each. The volume, velocity, and temperature of the Gulf Stream have been very variously estimated; and as to the sun's heat, when we remember how much the diathermancy of the air depends on its condition, we may not be able to accept with such confidence as Mr. Croll, the estimates of Pouillet; yet with every possible allowance, when the influence of a vast body of heated water is calculated, it will undoubtedly be much greater than would have been previously supposed, and actually amounts to a very considerable fraction, say $\frac{1}{10}$ of the whole of the sun's direct heat on the North Atlantic. Dr. Carpenter * brings objections against this method which render, in his opinion, the "figures" "utterly valueless." The first of these is that Mr. Croll does not give a correct account of the difference in temperature between the northern and southern hemispheres in assigning it to the transport of heated water by ocean currents; but it is obvious that the question as to where the Gulf Stream obtains its heat is entirely distinct from that as to its actual amount. The second objection, that since the temperature of the ocean is seldom more than 82°—86°, while the "direct heat of radiation" may amount to 215°; and therefore that "the heat lost by evaporation from the sea must be far greater than that lost by radiation from the land," is just one that shows the *value* of Mr. Croll's method. For, when treated in this way, the above figures show that the sea contains *more heat units* in its heated surface stratum than the layer of land that is influenced by the variations of surface-temperature, and that therefore the water at the equator is, as Mr. Croll states, the best adapted for retaining the heat of the sun, which is in reality no more than an elementary result of its high specific heat. Mr. Croll considers that the influence of the Gulf Stream is indirect, being manifested by the warming of the S.W. winds; and to the extent that he proves that the Gulf Stream raises the general temperature of the Atlantic he cannot be wrong. Were he to confine himself to the statement that the Gulf Stream and other ocean currents have a very sensible influence on the climate of the temperate regions, his position would appear to be impregnable against any who should represent its thermal effect "as very insignificant;" but when he adds that "ocean currents are the great agents employed" (to the exclusion of others) "in the distribution of heat over the globe," and estimates that the Gulf Stream alone raises the mean temperature of London 40°, he stands upon less certain ground. For these results depend on the following arguments:—(1) There is no ocean circulation but that by sensible currents; (2) The internal heat of the earth has no influence on climate; (3) The temperature of space is − 239° F.; and (4), the Gulf Stream supplies $\frac{1}{4}$ as much heat to the Atlantic as the direct rays of the sun. Of these arguments we will below discuss the first at length. The second is founded on a statement of Sir Wm. Thomson's, that an increase of temperature as great as 2° F. per foot in descending into the earth would not have an influence of more than 1° on the climate of the surface. This, however, means 1° over the present mean temperature, and in no way disproves that the internal heat of the earth does nothing in raising the temperature of its surface over that of space, an effect which it most certainly would have in a large degree. The third argument, as to the temperature of space, is therefore nothing to the point, and is moreover, as Mr. Croll himself admits,

* Proceedings of the Royal Society, June 13, 1872.

totally unreliable. We do not know the temperature *from* which the sun raises the earth, except that it is greater than that of space. The fourth argument, of course, is nothing without the first three, and the fraction $\frac{1}{8}$ we have seen may be much too large. We are not, then, in a position to estimate accurately the thermal effect of the Gulf Stream and other ocean currents; but we may consider it proved, as is indeed generally acknowledged, that they have a very sensible influence, and, as we shall see, bear a great part in the general circulation of the ocean water.

We must now examine how far Mr. Croll establishes his position that a general oceanic circulation is impossible under the influence of temperature and gravitation alone. Dr. Carpenter has already given (Proc. of Roy. Geog. Soc., vol. xviii.) his reasons for his belief in the adequacy of these influences, and his replies to Mr. Croll's objections, some of which are discussed in this volume in no less than four chapters. Although it may be familiar to most of our readers, it will be well to give here an outline of Dr. Carpenter's "doctrine."

The chilling of the salt water in both polar regions renders it heavier and causes it to sink, its place being supplied from the warmer water of lower latitudes, which is itself supplied by the motion of the water from the two poles towards the equator along the lower portion of the ocean; and these two masses meeting each other near the equator, well up there, and bring the colder water nearer the surface, while the heating of the surface water in these regions keeps up the difference between the specific gravities of the water supplied to and leaving the polar regions, on which the whole depends. These appear to be Dr. Carpenter's latest views (Proc. Roy. Geog. Soc., vol. xviii., June 1874), though Mr. Croll's objections seem, in some part at least, to be aimed at details that do not affect the fundamental conception. This is distinguished as a *vertical* circulation, because the first origination of the motion is supposed to be in the *descent* of the polar waters. Mr. Croll assents to the facts, but ascribes the circulation to the initiation of the winds, and denies that there is any circulation beyond that produced by currents. We know that currents exist on the surface, and it is generally agreed that they owe their origin, in great part at least, to the system of prevailing winds, and even on Dr. Carpenter's theory they must, so far as they tend polewards, decrease by so much the general circulation of the upper ocean; but the known or assumed under-currents are much more local, and the depression of temperature at great depths is too general to allow us to conceive that the return should be made by circumscribed currents.

In discussing the question whether the polar cold is sufficient to cause circulation, Mr. Croll first objects that the sea of the tropics is salter, and therefore denser, than that of the poles, and that this would counteract the effect of the cold. There is in reality but little force in this objection as against Dr. Carpenter's theory. The excess of temperature and of salinity counteract each other in the surface layers of the tropics, and prevent them sinking or rising; but as they have a nearly horizontal motion, according to the theory, the objection is nothing, the lower layers which alone have an upward vertical motion deriving it from a *vis-à-tergo*; and with regard to the polar area the lower layers cannot be more salt than the upper, from whence they come, according to the theory, and any *freezing* on the surface must leave the remaining water on the contrary salter.

The next objection of Mr. Croll is far more formidable, though it shows that some of the *proofs* adduced are untenable, rather than the theory itself. The drifting of icebergs from Newfoundland across the Gulf Stream, and of the Atlantic cable buoy which travelled six hundred nautical miles in seventy-six days, adduced by Dr. Carpenter as proving the southward motion of the deeper layers, proves too much according to Mr. Croll, as it proves the existence of a sensible *current*, which Dr. Carpenter admits cannot be formed by differences of gravity. This may be true, and prove that other causes operate in the motion of large masses of water; but while destroying one argument in favour of, it proves nothing in opposition to, the doctrine of general oceanic circulation. This class of objections, however, are far more forcible than theoretical ones; and the list of phenomena that may be accounted for on either theory, and of those that cannot well be accounted for on the gravitation theory, *e.g.* the southward currents of Davis Straits and the east coast of Greenland, shows that neither theory *alone* will satisfy all the conditions to be fulfilled. Mr. Croll, however, gives no satisfactory account of the greater cold of the lower strata of the South Atlantic, nor of the surging up of cold currents on eastern shores, nor of the cold water coming nearest the surface under the equator; nor does his theory give that beautiful account of the maintenance of life in the deep sea which is so dependent on the change of the water.

But Mr. Croll asserts that the gravitation theory is physically faulty, and maintains the assertion in this volume against Dr. Carpenter's last reply. In several of his arguments it is impossible not to agree with him. In examining them we will follow the order he takes. He first shows that heat at the surface, as in the equatorial regions, cannot produce circulation. But this, though essential to Lieut. Maury's theory, has not been asserted by Dr. Carpenter, who, on the contrary, states that any effect due to the heating at the equator may be practically disregarded; and why? because the heat is here applied at the top instead of at the bottom, as it should be to produce convection; but an application of cold to the top would be equivalent to heat at the bottom, and this cold is obtained in the polar area; consequently Dr. Carpenter regards polar cold as the *primum mobile*. Mr. Croll objects to this that it is the *difference* of temperature only we have to do with, and this may be said to depend on either, and accuses Dr. Carpenter with confusion of ideas; but this is scarcely fair after arguing against the heat being available to produce motion because applied at the top, showing that he perceived that not the difference *only*, but *where* the lower temperature is found is of consequence. Dr. Carpenter would say the temperature at the poles is *below* the average, no matter how that average is obtained; which is a very different thing from saying the equatorial temperature is *above* the average—since in the first case the average might be obtained, as far as the theory is concerned, by a nearly *uniform* temperature elsewhere.

The next important question raised with respect to this theory is the amount of force which is exerted to put the

water in motion. This Mr. Croll shows to depend entirely and only on the amount of slope of the water-surface from the equator to the pole, and not at all upon the amount of fall from the surface in the polar regions, to the lowest depth at which the water of maximum density s found beneath the equator. Dr. Carpenter says that this "would seem irreconcilable with the simplest principles of physics," a statement easier to make than to prove. For, as Mr. Croll shows plainly, the work done *by* gravity in the descent, is done *against* gravity in the ascent at the equator, and the two counteract each other, except only the extra amount of gravity which is called into action by the shrinkage of the polar column from what would have been its size under the *average* amount of solar heat, and which alone can have any *continuous* effect. The solar heat is a constantly supplied moving force which is used indirectly in the ocean circulation, and any further amount of gravity made use of in the circuit would involve the idea of perpetual motion. Connected with this is Dr. Carpenter's assertion that there is *no* difference of level between the equatorial and polar seas. Since, however, this is the *only* proximate cause for the ocean circulation, its denial would seem to cut the ground from beneath his feet. It will be found, however, that though he denies it in one place he asserts it in another, and his theory essentially depends on it. It is true that water *tends* to find its level when disturbed, as it is by the action of polar cold, which tends to alter its level; but it is just this *tendency* that causes the circulation. If one of the forces were to be powerful enough to have its own way entirely, no motion could occur; *i.e.* if the water were too viscous, a greater permanent change of level would arise; if it were *perfectly* fluid, the equilibrium would be brought about *instantaneously* and no visible motion would be perceived. We must be content, then, with the fall of level from equator to pole to produce the circulation: is it sufficient? This depends entirely on the viscosity of water. Mr. Croll bases his argument on the experiments of Dubuat, who showed that water would not descend a slope of 1 in 1,000,000, which is much greater than the slope under discussion, and hence the fall of level is too small to cause any circulation. He replies to Dr. Carpenter's objection, that these experiments had reference to water running over solids and not over itself, by saying that one layer of molecules alone would be in contact with the solid and the rest with the water surface only. The reply is plainly beside the mark, as Mr. Croll should have seen by reading Dr. Carpenter's statement following his objection, that the difference between a fluid restoring its own equilibrium, and having a sensible motion over solid surfaces, was well known in practice to Mr. Hawksley and other hydraulic engineers. But in reality no chamber experiments can determine such a point satisfactorily; and besides this, it seems to us that an important point has been overlooked by Mr. Croll. No doubt it would be hard for a single pound of water to perform its whole circuit against all opposing frictions under the impulse of the force due to so small an amount of slope; but if large masses of water move together, the moving force would be proportional to the mass, but the friction to be overcome would be simply that of the perimeter of the tube of flow, and it is an essential part of the theory of ocean circulation that the moving water is of immense mass. This friction would not increase, like statical friction, with the mass, since the pressure would be the same at the same depths, and it is also more of the nature of shearing force than friction, and therefore nearly a constant quantity.

It does not appear, then, that anything that has been said by Mr. Croll disproves this theory of a general oceanic circulation, though he may have successfully attacked it in certain respects. Nevertheless we agree with him that "if a vertical motion follows as a necessary consequence from a transference of water from the equator to the poles by gravity, it follows equally as a necessary consequence from the same transference by the winds; so that one is not at liberty to advocate a vertical circulation in the one case and to deny it in the other."

This was the opinion also of Herschel in his letter to Dr. Carpenter, that "henceforward the question of ocean currents will have to be considered under a twofold point of view." It would take too long to discuss the other points in which Mr. Croll enters into controversy with respect to various currents, such as the Gibraltar or the Baltic, and we must reserve for another notice the interesting points connected with past time with which the latter part of the book is occupied. J. F. B.

(*To be continued.*)

HILDEBRANDSSON ON UPPER ATMOSPHERIC CURRENTS

Essai sur les Courants supérieurs de l'Atmosphère, dans leur Relation aux Lignes Isobarométriques. Par H. Hildebrand Hildebrandsson. (Upsal, 1875.)

CLEMENT LEY, in "The Laws of the Winds prevailing in Western Europe," expresses his opinion, based on observations made near Hereford on the movements of the cirrus cloud, that in general the upper currents of the atmosphere flow away from the regions of low atmospheric pressure, and converge upon regions of high pressure. This being a point of the utmost importance with reference to the general circulation of the atmosphere, M. Hildebrandsson, in December 1873, organised a systematic observation of the cirrus cloud in Sweden. Twenty of the Swedish meteorological observers engaged in the work of observation, the network of stations extending over nearly 11° of latitude, from Tomarp in the south to Qvickjock in the north. The above essay discusses these observations with great ability.

The question of the relation of the motions of the cirrus cloud to areas of high and low pressure is cleverly handled in the essay, and the method of discussion, illustrated by thirty-three charts, may be referred to as a satisfactory and exhaustive treatment of the data of cirrus observation, which are restricted only to one country. Charts I. to VIII. represent well-selected instances of storms advancing on Sweden from westward; Charts IX. to XVI. represent Sweden in the rear of storms; and Charts XVII. to XXIV. represent areas of high pressure in various directions, S., E., &c., from Sweden. Comparing the direction of the upper currents with these areas of high and low pressure, it is shown that quite near the centre of the depression area of storms the upper currents blow in directions nearly parallel to the isobars and to the winds on the surface of the earth, but that in

proportion as we proceed from the centre they are turned outwards, being deflected to the right of the surface winds; in other words, they tend more and more to blow out from the area of low pressure. On the other hand, they converge upon the centre of the regions of high pressure, cutting the isobars nearly at right angles. This last point is interesting in connection with the circumstance pointed out some time ago by Hoffmeyer, that surface winds in blowing out of the areas of high pressure cut the isobars approximately at right angles. Charts XXV. to XXXII. are selected to represent instances in which Sweden lies between two storms, the one following the other with only a short interval between them. In these cases the behaviour of the upper currents from both storms and the manner in which they blend together at their contiguous margins are very instructive.

The winds on the surface of the earth, as compared with the upper currents, show, as is well known, inverse relations to areas of low and high pressure—blowing inwards upon areas of low pressure, and outwards from areas of high pressure. Consequently, as the author remarks, an area of low barometer is necessarily the region of an ascending current, which, when it has risen to a great height in the atmosphere, flows away from the central space of low pressure towards regions of high pressure, whence it sinks gradually down to the surface as a descending current, and in this manner a vertical circulation is constantly maintained between the surface of the earth and the higher limits of the atmosphere. We very strongly recommend that, as has been so successfully carried out in Sweden, a thorough and systematic observation of the cirrus cloud be generally inaugurated in other countries, so that it may be possible to chart the upper currents over a wide extent. Among the many points suggested by M. Hildebrandsson's charts is the question whether the extent and volume of the upper currents flowing outwards from storm areas be consistent with some of the views recently advanced on the theory of storms and circulation of the atmosphere. We hope meteorologists will soon take steps to occupy the important field of observation now opened up.

OUR BOOK SHELF

The Zoological Record for 1873. Edited by E. C. Rye, F.Z.S. (London: J. Van Voorst, 1875.)

IN the preface to the "Record" for 1872 Prof. Newton, the editor, announced that having intimated to the Zoological Record Association his intention to resign his post, the Council had appointed Mr. Rye, Librarian to the Royal Geographical Society, as his successor. From a glance into the present volume it is evident that it is Mr. Rye's intention to maintain the high standard of his predecessors, notwithstanding the difficulties he has had to encounter, especially in the loss of the services of Dr. Günther, whose increased duties, now that he has been promoted to the post of Keeper of the Natural History Department of the British Museum, prevent him from undertaking the *Mammalia*, *Reptilia*, and *Pisces*, as he has done for years. Mr. Rye has succeeded in obtaining the services of Mr. E. R. Alston, F.Z.S, on the Mammals, and of Mr. A. W. E. O'Shaughnessy on the Reptiles and Fishes; both which naturalists have most creditably performed their laborious tasks. Mr. R. B. Sharpe has undertaken the Birds as before, whilst Dr. Ed. von Martens, the Rev. O. Pickard-Cambridge, Mr. Rye, Mr. Kirby, Mr. McLachlan, and Dr. Lütken, have devoted themselves to their special subjects. The editor acknowledges the grant of 100*l*. from the British Association, 50*l*. from the Zoological Society, and 100*l*. from the Government Grant Committee of the Royal Society (this being the first occasion that the Record Association has been so assisted), towards the expenses of publication. The increasing necessity for the production of the volume is yearly becoming more evident, at the same time that its contents are necessarily of such a nature that there can never be a demand for it which will enable it nearly to cover its expenses. The most important scientific results of the year include the investigations of Leidy, Marsh, and Cope on the fossil American Eocene Mammalia, and Prof. Marsh's discovery of a new sub-class of fossil toothed birds, respecting which all naturalists cannot but regret that so little opportunity is given them of seeing specimens or even drawings of the great number of species now known to them by short descriptions only.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*]

Systems of Consanguinity

I AM sorry to find that on some points I have misunderstood the views of my friend Mr. Morgan (vol. xii. p. 86), and the more so as, after reading his letter very carefully, I am not sure that I quite comprehend them even now. Your reviewer is no doubt able to reply for himself: but it certainly seems to me not remarkable that both he and I should have been led into error. Indeed, I do not exactly understand whether Mr. Morgan intends to say that we have misapprehended his views in supposing that in his opinion one of the two great systems of classification of relationships is "arbitrary, artificial, and intentional." Mr. Morgan admits that he himself used these terms in several places. There are, he says, "three or four places, and perhaps more, in that volume in which I speak of the system of a particular people as 'artificial and complicated,' and as 'arbitrary and artificial,' without the qualification in each case which should, perhaps, have been inserted." Thus your reviewer and I were, as he himself allows, using his very own words, though I shall of course omit them if my book should reach a fourth edition.

Moreover, these descriptive epithets are not used casually, but form the very basis of his argument. For instance, in p. 469 he says:—

"It may be remarked, however, that if the system is to be regarded as exclusively natural and spontaneous, the argument for unity of origin would be without force; since, as such, it would be the form to which all nations must insensibly gravitate under the exercise of ordinary intelligence. But if to reach the descriptive system these families have struggled out of a previous system, altogether different, through a series of customs and institutions which existed antecedently to the attainment of the state of marriage between single pairs, then it becomes a result or ultimate consequence of customs and institutions of man's invention, rather than a system taught by nature." *

But then, as I understand, he alleges that a different theory is given in his concluding chapter. So far, however, from finding in that chapter any indication of a change of opinions, I see that he reiterates the same view. After discussing the classificatory system, he says: "There would seem to be but four conceivable ways of accounting for the joint possession of this system of relationship by the Turanian and Ganowánian families; and they are the following:—First, by borrowing from each other; secondly, by accidental invention in disconnected areas; thirdly, by spontaneous growth in like disconnected areas, under the influence of suggestions springing from similar wants in similar conditions of society; and fourthly, by transmission with the blood from a common original source." †

After negativing the two first hypotheses, he proceeds to discuss the third, namely, that of "spontaneous growth under the influence of suggestions springing from similar wants in similar

* Morgan's "Systems of Consanguinity and Affinity of the Human Family," p. 469.

† Ibid. p. 500.

conditions of society." This possible theory, he says, "has been made a subject of not less careful study and reflection than the system itself." But after a patient analysis and comparison of its several forms, he comes to the conclusion that it is insufficient to account for the facts.

Thus, as it seems to me, he clearly repudiates the theory of spontaneous growth.

Mr. Morgan thinks that his solution of the problem of relationships must have escaped my notice, because I did not discuss it in my paper read before the Anthropological Institute; but in that memoir I quoted from the chapter in question, and went on to say—

"Mr. Morgan admits that systems of relationships have undergone a gradual development, following that of the social condition; but he also attributes to them great value in the determination of ethnological affinities. I am not sure that I exactly understand his views as to the precise bearing of these two conclusions in relation to one another; and I have elsewhere given my reasons for dissenting from his interpretation of the facts in reference to social relations."

Thus I expressly pointed out that Mr. Morgan, while characterising the "classificatory" system, to use his own terms, as "arbitrary and artificial," nevertheless also regards it as having "undergone a gradual development following that of the social condition." Surely Mr. Morgan must have written his letter without having my book by him, for it seems to me that the above passages, taken together, represent his own theory, as given in his letter. Mr. Morgan hints that the conclusions contained in his last chapter had escaped my notice. He appears to have overlooked the fact that I quoted from that very chapter. I was not, however, reviewing his work, and differing fundamentally, as I do, from the conclusions adopted by him, while feeling deeply also the great obligations to him under which ethnologists lie, I preferred to state my own views rather than to dwell on the differences between the conclusions at which he and I have arrived. JOHN LUBBOCK

Down, Kent, June 7

Attraction and Repulsion caused by Radiation

I DID not intend to reply to Prof. Osborne Reynolds' letter in NATURE, vol. xii. p. 6, but some persons expect me to say something about it. If the Professor would be careful not to answer me with the ideas that occur to him as he is "on the point of sending off the paper" (see Phil. Mag., Nov. 1874), he would save himself the trouble of many explanations. After my thousand experiments it is scarcely respectful to try to overcome all by his few, and, after three years of my thought, rather hasty to tell me that he explained it all so suddenly with perfect certainty, and that I am unable to comprehend him. It is also scarcely wise to lead us to infer that probably he cannot explain the whole, but that he knows somebody who will soon do it.

Prof. Reynolds seems to base his calculations on some of my experiments which dealt with a perceptible amount of gas, and has not taken notice of those where there is no amount of gas known to be present; for example, in a chemical vacuum.

Prof. Reynolds must show that there is gas or vapour remaining, and he must also show that there is enough to produce the mechanical results. He tells us that the forces will increase as the density of the gases diminishes. The speed will, but if the force does, that can only be up to a certain point, when it is equally certain that a change will take place, and the motion of the particles or molecules will be attended with less force according as they diminish in number. The opposite to this involves something not intended. I suppose he does not intend to speak of forces without matter. The analogy with sound is not quite happy, as that is so readily diminished by lower pressure; although the speed is the same, the power is small. Besides this, what will he say to the case where there is no heat and only light? I am abundantly willing to allow molecules and forces, but I see no place for such as I have been acquainted with.

I am working at the subject and shall be glad to come to a true conclusion. Scientific men need not be so very much afraid of a new law of nature, for some are wanted, and there are certainly many yet waiting to be discovered before nature becomes intelligible to us.

I by no means deny that the phenomena are connected with molecular movements, but I believe that Prof. Reynolds has neither explained this nor proved it by experiment. His explanation suits only a part of my work; and so does the saying that he "experiments stand in much the same relation to the kinetic theory of gases that Foucault's pendulum occupied with regard to the rotation of the earth." This is an analogy showing much acuteness, viewing the matter from what I consider the unproved side.

Prof. Reynolds goes far when he says that my experiments are "the only direct proof that has ever been obtained of the kinetic theory of gases." It may be, but if so, physicists must have been too easily pleased with their theories.

I might say much more, but I prefer to wait. There is but little good done by short notes when such a large and important subject waits for elucidation. WILLIAM CROOKES

London

American Indian Weapons

THE Pai-ute weapon, described by Mr. Mason in your last number (p. 107), although extremely interesting and quite new to me, appears scarcely sufficiently characteristic of a war weapon to form an exception to the statement of Schoolcraft, that the clubs of the North American Indians as a rule are curved. It would be interesting if it could be ascertained how such a peculiar instrument as that described by your correspondent came to be used as a weapon of war. Its form precludes the possibility of its having been designed for such a purpose. The mode of holding it suggests the idea of its having originally been used as a pounder, the thick end having perhaps been employed for pounding grain, beating out grass for cloth, or for preparing skins. It somewhat resembles the instrument used for making bark cloth in some of the Polynesian Isles, and it corresponds to the Beatle (Battelle) still used by Irishwomen for beating flax, and occasionally, I have no doubt, as a weapon of war; but these are used with the flat side, not the end. The only weapon I know of that is used like the Pai-ute club is the New Zealander's Merai or Pattoo-Pattoo, the sharp end of which is thrust into the back of the head of the offender; and I have suggested elsewhere that this peculiar and awkward mode of using it arose from its having been originally what its form resembles, a stone axe blade (celt), used as the Australians now use it sometimes, in the hand without any handle. The sharp edge at the end of the Merai shows its original intention, in the same way that the flat end of the Pai-ute club could never have been designed as an offensive weapon, but would have been useful as a pounder; it may be, in fact, a "survival" converted to other uses. There exists, of course, no law of nature to prevent North American Indians from using straight clubs as well as curved ones, but my observation of their weapons confirms the statement of Schoolcraft, that as a rule they do not. Amongst races in a more primitive state of culture, as amongst the Australians, we find that nearly every form of club that is made straight is used also in a curved form, the curvature arising merely from the natural bend of the branch out of which it was constructed; when these natural curves were found useful, they appear to have been retained and systematised. But the North American weapons are of a more advanced and conventionalised description, and we cannot trace their origin and *growth* so clearly as amongst lower savages. The description of the Moquis boomerang by Mr. Mason is an interesting fact, which, combined with the mention of it by Bancroft amongst the Pueblo Indians of New Mexico, points to the probability of a connected area of distribution. Drawings of weapons such as those given in your journal are of the utmost value in assisting to trace the distribution of like forms. A. LANE-FOX

Guildford, June 12

Hardened Glass

PERHAPS the following short and preliminary account of some observations on the optical and mechanical properties of De la Bastie's toughened, or, as I think more correctly, hardened glass, may interest your correspondent Mr. James H. Logan (vol. xii. p. 87).

Immediately after the publication of M. De la Bastie's specification and prepared specimens of the glass, I submitted them to careful optical examination by polarised light. Perhaps the best experiments are those made by means of short cylinders and small cubes and parallelopipeds carefully "hardened." A small cube with half-inch sides thus prepared has its sides ground plane and polished. The operation of polishing may be dispensed with if a small microscopical thin cover be cemented on the ground surface with Canada balsam. The cube is then mounted between strips of blackened cork, and examined in the

usual way by means of Nicol's prisms of glass plates or other appropriate polariscope. The beautiful chromatic phenomena thus brought out at once indicate that amongst the causes which operate to produce the hardness of glass, powerful compression of the interior by the contracting exterior must be one. The phenomena are, in fact, essentially those of compressed glass, and the curves of colour, or black and yellow, seen when the glass is examined by white or monochromatic light, indicate successive curves of tension and balanced or no-tension. In a carefully prepared glass rod of half-inch length these curves are rings traversed by a well-marked black cross. In an oval the rings assume the character of those seen in braxial crystals. When plates are examined, the light being transmitted from back to front, they appear to act essentially as bi-refracting plates with crosses and bands somewhat irregularly distributed, but capable of being referred to the angles of the places or to centres of unequal heating.

My experiments on the mechanical properties of the glass have chiefly been confined to testing its hardness and the possibility of grinding it. So far as I have gone at present I make it to be nearly twice as hard as ordinary glass, which it scratches with ease. It can be cut with a good file well moistened with turpentine, and can be ground on a stone with sand, without fracturing, if great care be taken and the glass be well prepared. One piece, which manifested when under the polariscope evidences of ill-balanced tension, the neutral line lying near one surface, submitted to transverse grooving, but disintegrated on being ground on one surface as soon as the outer surface had been ground away to near the neutral line. There appears to be an easily reached limit beyond which the surfaces must not be unequally removed, but as my friend Mr. Thos. Fairley, F.R.S.E., has been good enough to show me, there is practically no limit beyond which *both* surfaces may not be *simultaneously* removed. This result, foretold by me from polariscopical analysis, Mr. Fairley has kindly shown by dissolving the opposing surfaces away by hydrofluoric acid. The least hard portions dissolved much more readily than the thoroughly hardened, and the etched surfaces show wavy lines closely following the tension lines shown by the polariscope. There is further this remarkable feature, that the inner portion of the glass proves to be essentially common glass, which fractures in the ordinary way. Further experiments are necessary for the complete elucidation of the subject, and are in progress, but the preceding may be useful to fellow-workers on the subject.

Leeds, June 12 HENRY POCKLINGTON

The House-fly—A Query

IN one of the rooms in the Science Schools lately built here, I have noticed, in the last week or so, great numbers of the large house-fly (*Musca domestica*) lying dead on the floor. Last Tuesday I saw one fall dead, but this is the only one. This morning I counted thirty-two in a space of about three square yards. I examined one under a microscope, and found that most of the small hairs on its body were covered with a yellowish powder. Can any of your readers give me any explanation of this?

Harrow, June 8 HARROVIAN

OUR ASTRONOMICAL COLUMN

VARIABLE STARS.—Mr. J. E. Gore (Umballa, Punjâb) writes, under date May 5, that he believes 27 Canis Majoris to be a variable star. It is 4 in Harding's Atlas, but at present about 5½ or 6, and much inferior to 28 in the same constellation, which Harding rates at 5. The change of brightness was first noticed in 1874. This star is 4·5 in the Radcliffe Catalogues, 5 in Arg. Zones, 5½ in Lacaille, and 6·5 in Heis's Catalogue; Behrmann has 6, and the lowest estimate of magnitude is 7, in Flamsteed's Catalogue, with respect to which Baily remarks that there is no magnitude recorded in the original observation-book, and that modern observations make it 4½. Mr. Gore states he has also "suspected some variation of light in the red star 22 Canis Majoris (between δ and ϵ); it is usually rated as of magnitude 3 or 3½, but for some time past it has seemed rather fainter than an ordinary star of the fourth magnitude." Bradley and Piazzi have this star 3·4, Flamsteed, Brisbane, and Heis, 4, the Washington General Catalogue 5, and it is so rated once by Argelander; in Behrmann it is 4·5.

We will take this opportunity of directing the attention of our astronomical readers in the southern hemisphere to Behrmann's valuable Atlas and accompanying Catalogue, which, pending the publication of Dr. Gould's Cordoba "Uranometria," is the only real authority for recent magnitudes of the naked-eye stars of the southern heavens. It is entitled "Atlas des Südlichen Gestirnten Himmels, von Dr. Carl Behrmann" (Leipsic, 1874), and contains the stars in forty-six constellations between 20° of south declination and the south pole, and is arranged upon the plan of Argelander's well-known work. The number of stars included in the Atlas is 2,344. It was formed by Behrmann in the short space of from nine to ten months, beginning in the autumn of 1866, and on that account, as the author remarks, there may probably be some omissions and errors, but it is nevertheless a very meritorious and important work. It appears, from Dr. Gould's report to the Minister of Public Instruction of the Argentine Republic, that his "Uranometria" has undergone the intended revision, and is now completed, and that steps are being taken for its publication. It is only one of the extensive scientific undertakings which will mark the residence of this distinguished and energetic astronomer at Cordoba.

THE BINARY STAR η CORONÆ BOREALIS.—Mr. Wilson, Temple Observatory, Rugby, has published some remarks upon the tendency of recent measures of this star to shorten the period of revolution assigned by computers hitherto, and refers to Winnecke's careful discussion of the measures to 1856. Winnecke's orbit, however, is not the latest that has been calculated, that of Wijkander including measures to 1870, and the period he finds, 41·58 years, is not much different from that which Mr. Wilson considers to be required by the more recent measures. Still, these later observations point to a further diminution of the period, the exact amount of which may probably be soon determined. The following angles and distances are calculated from Wijkander's orbit, and on comparison of the former with the results of observation, it will be found that the computed value is now about 3° behind the true one.

1872·0	Angle	48°·07	Distance	0″·90
73·0	,,	51 ·98	,,	0 ·86
74·0	,,	56 ·35	,,	0 ·81
75·0	,,	61 ·32	,,	0 ·76
76·0	,,	67 ·05	,,	0 ·70

This orbit gives the angle too small by 5°·3 for Sir W. Herschel's measure in 1782, and also too small by 4°·3 for his measures in 1802, or, if these differences are expressed in the form Δ sin. d P, $-0''{\cdot}09$ and $-0''{\cdot}04$ respectively.

Sir W. Herschel's description of his experience with this star is found in *Philosoph. Trans.* 1804. On Sept. 9, 1781, the position was 59° 19′ *n.f.*, and on Sept. 6, 1802, by "a mean of two very accurate measures" it was 89° 40′ *n.p.* (This is now found to require correction of 180°.) Herschel further states "the distance of the two stars has not been subject to any sensible alteration. Sept. 9, 1781, a very small division might be seen with 460. Aug. 30, 1794, they were so close that with a 10-feet reflector and power of 600 a very minute division could but just be perceived. April 15, 1803, with a 10-feet reflector, a very small division was also visible, with 400, though better with 600. And May 15, 1803, I saw the separation between the two stars with the same 7-feet reflector and magnifying power of 460, with which I had seen it twenty-two years before." We have from Wijkander's orbit for comparison with this account:—

1781·69	Angle	25°·4	Position	0″·98
1794·66	,,	80 ·2	,,	0 ·60
1802·68	,,	175 ·5	,,	0 ·57
1803·37	,,	181 ·6	,,	0 ·59

Except in 1781, it will be remarked, the distances at the

dates of Herschel's observations are given sensibly the same.

PROPER MOTION OF B.A.C. 793.—Prof. C. P. Smyth has lately drawn attention to an apparent variation in the amount of proper motion of the star B.A.C. 793, shown by the Edinburgh observations between 1837 and 1868, involving a diminution in the motion in R.A. and an increase in that in N.P.D. The star is No. 31 of the list included in Argelander's *Untersuchungen über die Eigenbewegungen von* 250 *Sternen*, Bonn Observations, Vol. vii., Part I., where, from a rigorous discussion of seventy years' observations, the proper motion in R.A. is found to be + 0·1245s, and that in N.P.D., – 1″·456. The comparison of the normal place for 1855·0 with the whole course of published observations to 1865, in which every refinement of calculation is introduced and the above proper motions employed, with Bessel's precession-constants, does not afford any indication of the variability of proper motion suspected by Prof. Smyth. The last Edinburgh observations in 1866 and 1867 show a difference from Argelander's formula of only – 0·08s. in R.A., and agree exactly with the N.P.D. The Washington position, depending upon two observations towards the end of 1870, is in close agreement with Argelander in R.A., and differs – 2″·0 in N.P.D. If a position of the star depending upon a good number of observations should be obtained during the present year, the point may be definitively settled, but thus far variation of the proper motion appears to be at least questionable. Upon this subject see Bonn Observations as above, pp. 20, 54, and 109.

MINOR PLANET NO. 146.—The number of small planets is rapidly approaching *one hundred and fifty*. M. Borrelly, of the Observatory at Marseilles, announces his discovery of No. 146 on the evening of June 8. At 10 P.M. its place was in R.A. 17h. 20m. 16s., and N.P.D. 111° 20′ 15″; it is as bright as stars of the eleventh magnitude, and therefore for the present should be readily identified by means of Chacornac's Chart No. 52.

SCIENCE IN GERMANY

(*From German Correspondents.*)

HERR VON BEZOLD, of Munich, has published some interesting researches on the periodical changes in the frequency of thunderstorms during long periods of time. These researches are particularly noteworthy for the original manner in which the author has used the statistical materials on thunderstorms which he could obtain (principally within the kingdom of Bavaria). As the character of our reports will not permit us to give details with regard to the manner of treatment, we pass at once to the results which Herr von Bezold has arrived at.

First of all it was found that the frequency of thunderstorms during a long period is generally either on a continuous increase or decrease, and that these variations are periodical.

If we ask on which other meteorological phenomena these variations could possibly depend, the first thing to be considered is the temperature. It is further advisable, on account of the numerous relations that have lately been discovered to exist between sunspots and meteorological phenomena, to turn attention also in this direction. It has been found in reality, that if we represent the variations of the frequency of thunderstorms by a curve and compare the same with the curve of the frequency of sunspots, the minima of the thunderstorm curve coincide exactly with the maxima in the sunspot curve. On the other hand, the thunderstorm curve forms, to a certain extent, the mean between the sunspot curve and the curve of the deviation of the average yearly temperature for our latitudes.

We must observe here that although the path of the thunderstorm curve shows a general and unmistakeable connection with that of the sunspot curve (so that, for instance, for the period from 1775 to 1822 the maxima of the thunderstorm curve coincide almost completely with the minima of the sunspot curve), yet the details of the thunderstorm curve coincide better with the details of the curve of temperatures, so that nearly every rise or fall in the latter can be distinctly traced in the former. This connection between thunderstorms and the deviations of the yearly temperatures from the total average, shows itself still clearly, even where that between the thunderstorm and sunspot curves is less apparent.

Herr von Bezold recapitulates the results of his investigations as follows:—High temperatures, as well as a solar surface free from spots, cause a greater number of thunderstorms during a year than the reverse. Now, as the maxima in the frequency of sunspots coincide with the maxima of the intensity of aurora borealis, it follows that both groups of electrical phenomena, thunderstorms and auroræ, complement each other, as it were, so that in years with many thunderstorms auroræ will be rare, and *vice versâ*.

From this connection between sunspots and thunderstorms an immediate electric action between the earth and the sun does not necessarily follow, but it may be simply a consequence of the magnitude of insolation, which depends on the frequency of spots. These changes in the insolation are not felt simultaneously but successively in the different latitudes. The phenomena of thunderstorms, however, do not only depend on the conditions of temperature at a given locality, but also on the state of the atmosphere at far distant points, belonging to another zone; and this is most evident with thunderstorms accompanying strong currents of wind or tempests. In this manner the peculiar intermediary position which the thunderstorm curve occupies between the curves of temperature and sunspots might perhaps find its explanation eventually.

IN zoological investigations experiments are rare, and therefore the results obtained by them are all the more valuable. The latest work of this kind—"Researches on the Theory of Descent: I. On the Season-dimorphism of Butterflies," by Dr. August Weismann, Professor at Freiburg—will, however, interest not only the narrower circle of entomologists, but also the amateurs in this branch of science, as it will furnish them with a sort of guide for the pursuit of their hobbies in such a manner as to do great service to science. Weismann bases his researches on the fact, which has been known for some time, and which has been called "season-dimorphism" by Wallace, that certain butterflies, when issuing from their winter chrysalis in the spring, show a different coloration and design upon their wings than do those which appear in the following summer; so that until this fact was discovered, the two forms were thought to be two distinct species of butterflies. We will only mention one of many examples, as it refers to one of the commonest kinds of day-butterflies. *Vanessa levana* is only the winter form of *Vanessa prorsa*, which is the summer form produced by the former; the latest offspring of the latter, which survive the winter, reappear as *Vanessa levana* in the following spring. Weismann exposed the caterpillars produced by *V. levana* in May, which in the normal state should have produced the imago of *V. prorsa*, to a continuous temperature of 0° – 1° C., after they had changed to nymphæ. The result was that they yielded the winter form *V. levana*, with few exceptions. The same result was obtained with the second summer generation, which under ordinary conditions would still have appeared as *V. prorsa*. On the other hand, Weismann succeeded only very rarely in forcing the last generation in the year again to take the *Prorsa* form, by keeping the nymphæ in hothouses at 15° – 30° C., instead

of in the ordinary winter temperature. Most of the nymphæ passed the winter even in hothouses or in heated rooms, and produced *V. levana* in the spring. Similar researches were made by Weismann with another common day-butterfly species, *Pieris napi*.

Weismann thinks that the winter form of these butterflies was the original one, which existed alone and in a single annual generation in Europe, during the so-called ice period. As the summers became longer and warmer, a second and finally a third annual generation could be produced, and these were changed to the *Prorsa* form by the higher temperature. The return of the colder season then always caused a return to the original form (*Atavism*), just as it occurred in the experiments. To confirm this view, Weismann quotes the fact that in Lapland and in the upper Alps only a winter form of *P. napi* exists. As with an incomplete return to the original form intermediate forms result, the varying aspects of which prove that the change of the original form always takes place in a certain direction, Weismann thinks that the change of temperature might certainly have given the impulse for a change of form, but that the particular direction of the same lies in the constitution of the animal in question. We may certainly consider as a result of these investigations, that a change of climate, together with other causes, may have directly produced a great number of different species of butterflies.

Another fact mentioned by Weismann refers to the above, and is no less interesting. There is one of the lower Crustaceæ, *Leptodora hyalina* (Siebold's and Kölliker's *Zeitschrift für Wissenschaftliche Zoologie*, 1875), which is remarkable in many ways. This animal, according to the observations of the Norwegian Sars, shows similar phenomena, as the winter breed is differently developed from the summer breed, although the perfect forms are not so widely different as those of the butterflies.

ZOOLOGICAL NONSENSE

NOT many months since a controversy which had been raging for several weeks in the columns of the so-called "leading journal" was suddenly and completely put an end to by a well-known writer in a contemporary calmly and dispassionately pointing out that both disputants had been uttering what was absolute nonsense. "I use the word nonsense," he went on to say, "not as it is often used as a vague term of disapproval, but with a strict specific meaning, as contradistinguished from sense. All words—all articulate words—must be either sense or nonsense. They are sense if their meaning can be imagined, conceived, represented in some way or other to the mind. They are nonsense if their meaning cannot be imagined, conceived, or represented in any way to the mind. When a man says, 'I saw six men and two women walking down such a street, dressed in such a way, and heard them talking on such a subject,' anyone can understand, whether he believes it or not. The speaker is talking sense, whether truly or falsely. If he were to say he saw two crooked straight lines standing in the five corners of a square, you would say he was talking nonsense, that his words were neither true nor false, and that he might as well keep silence, or utter any other unmeaning sounds. The difference between these two examples consists solely in this, that the first assertion can, whereas the last cannot, be pictured to the mind. Each particular word by itself is as clear in the one case as in the other."

What the question then under discussion was, does not signify. Enough that it was nothing which had to do with natural science. But we are sorry to say that nonsense is still occasionally spoken or written by those who, if they do not exactly profess to be scientific, yet pretend to treat of things that clearly belong to the domain of science, and so make some approach to that character. Moreover, they are looked up to by some well-meaning though imperfectly instructed persons as authorities worthy of consideration. There was a time when there was a good deal of nonsense written by naturalists, and especially by zoologists, but we had been in hopes that the practice was entirely given up. It seems, however, that we are disappointed. Here is a melancholy instance to which our attention has lately been called :—

"I have never seen any reason to doubt, *first*, that the Vertebrata, or more properly 'Endosteata,' are the central group of the animal kingdom, the others being the Exosteates (or Articulates), the Anosteates (or Molluscs), and the Actiniates (or Radiates); *secondly*, that the Sucklers are the central group of Endosteates, the other groups being Birds, Reptiles, and Fishes; the Sucklers are connected with Birds through the Bats, with Reptiles through Pangolins and Armadillos, and with Fishes through Porpoises and Whales. The pectoral sucklers (Primates) are central, and MAN is the centre of these—not a mere unit on the circumference of the system."

There is no need to name the writer of this passage or the publication in which it appeared within the last few weeks, because our business is with the matter, not with the man, though we can hardly do otherwise than marvel at his style of easy assurance—"I have never seen any reason to doubt." We at first almost fear a platitude, then catching a glimpse of what is coming, we begin to think we are on the verge of a great discovery, or perhaps shall be brought face to face with intelligence itself. Sad is our disappointment as the sentence proceeds. The unwonted word "Endosteata" jars our bones within us, but we recover as we best can, and so far suppose it is all right; the expression of a "central group" may pass as a metaphor, and we feel a sense of relief and obligation at having the extraordinary names of the other groups translated for us; but then we thought we had somewhere been taught the Radiates had no existence. However, we hail a friendly semicolon, and find that we are arrived at the end of the author's first article of faith, which, though obscured by the metaphor, is yet intelligible. Now, then, for his "secondly." The word "Sucklers" strikes us as singular, but we discover that whatever it means forms another "central group," this time of "Endosteates"; so, to meet metaphor by metaphor, we exclaim "wheels within wheels," and it is a comfort to find that the surrounding groups are our old friends Birds, Reptiles, and Fishes; Amphibians, we suppose, being packed between the two latter. The next part of the sentence, however, is absolutely shocking: "Sucklers" connected with Birds through Bats, with Reptiles through Pangolins and Armadillos, and so on. Why, what is a zoological connection? Is it of affinity or analogy? Can the author have ever seen or examined the structure of the animals he mentions? We are taken back to the dark ages of zoology, if not to ages almost prehistoric. Needless to say that our confidence is gone. Then we have the concluding sentence with the old metaphor once more, and a new one; or is it that no metaphor is intended after all? that these concentric circles forming a system with a circumference on which man is *not* a unit —we wonder who ever said he was—exist in the author's mind? In our own we are free to say they do not. We are sure that they do not exist in nature, and we are so unimaginative that we cannot picture a representation of them to ourselves. Accordingly there is no help for it but to conclude that all this is clear, unmistakable, undeniable nonsense, as much so as the two crooked straight lines standing in the five corners of a square. These "circles," with their unit-bearing circumference, are, in the words of the writer from whom we first quoted, "the nonsensical shreds of exploded metaphysics"—relics of that silly "circular system" with its mystical numbers, its fives or its sevens—the will-o'-the-wisp of fancy that once

led men's minds astray from the path where only they could find the truth they were earnestly seeking.

Those who desire to believe nonsense at all hazards and in the face of the clearest possible proofs, and indeed like it rather the better because it is so, can of course continue in their fool's paradise. Who can doubt that they see the paragon of animals in the author of the passage we have been criticising, and that he sits at the centre—the "focal point" is the choice expression, we believe—of a select circle of admiring "pectoral sucklers" the very "hub of the universe," as our American friends might say? The Report of the last Local Examination Syndicate of one of our Universities speaks of Zoology as follows:—"The general character of the work in this subject is, perhaps, even worse than it was last year. In many cases the teaching appears to have been faulty or defective; there was a general ignorance of the principles of zoological classification; and a great number of candidates sent up answers so full of confusion and error as to lead to the opinion that they had only prepared for the examination by a hurried attempt to learn portions of a text-book by rote." Who can wonder at this prevalent "ignorance of the principles of classification" when a zoologist in a position to give instruction to youth and encourage their devotion to the study of nature utters absurdities such as we have just been noticing? We fear that he is not alone in his mischievous folly.

*LECTURES AT THE ZOOLOGICAL GARDENS**

VII.

June 10.—*Prof. Mivart on Kangaroos.*

AFTER pointing out the external and osteological characters of the Kangaroo, the lecturer proceeded to consider the question, What is a Kangaroo? what its place in the scale of animated beings; as also its relations to space and time? At birth the Kangaroo is strangely different from what it ultimately becomes. It is customary to speak of the human infant as exceptionally helpless at birth and after it, but it is at once capable of vigorous sucking, and very early learns to seek the nipple. The great Kangaroo, standing some six feet high, is at birth scarcely more than an inch long. Born in such a feeble and imperfect condition, the young Kangaroo is not able to suck of its own accord. The mother places it on one of the nipples and squeezes its own milk-gland by means of a muscle which covers it, in such a way that the fluid enters the mouth of the young one. In most animals, man included, the air-passage opens into the floor of the mouth behind the tongue, and *in front* of the opening of the gullet. Each particle of food as it goes towards the gullet passes over the entrance to the windpipe, but is prevented from falling in by the action of the epiglottis, which stands up in front of the opening and closes over it when food is passing. But in the young Kangaroo, the milk being introduced, not by any voluntary act of the recipient, but by the action of the mother, it is evident that some special mechanism is necessary to prevent choking. This is found in the elongation of the upper part of the windpipe, which projects up into the nasal passage, and is embraced by the soft palate in such a manner that the food passes on each side of it, whilst the air does not enter the mouth at all.

The Kangaroo browses on the herbage and bushes of more or less open country; and, when feeding, commonly applies its front limbs to the ground. It readily, however, raises itself on its hind limbs and strong tail, as on a tripod, when any sound, sight, or smell alarms its natural timidity. Mr. Gould tells us that the natives sometimes hunt them by forming a great circle around them, gradually converging upon them and so frightening them by cries that they become an easy prey to their clubs. The Kangaroo is said to be able to clear even more than fifteen feet at one bound. It breeds freely in the Society's Gardens, many being reared to maturity. They have been also more or less acclimatised in the grounds of Glastonbury Abbey, in the parks of Lord Hill and the Duke of Marlborough, and elsewhere.

* Continued from p. 114.

It is just upon one hundred and five years since the Kangaroo was first distinctly seen by Englishmen. At the recommendation and request of the Royal Society, Capt. (then Lieutenant) Cook set sail in 1768, in the ship *Endeavour*, on a voyage of exploration, and for the observation of the Transit of Venus of the year 1769. In the spring of the following year the ship steered from New Zealand to the eastern coast of New Holland, visiting, among other places, Botany Bay. Afterwards, when detained in Endeavour River, an animal as large as a greyhound, of a slender make, a mouse colour, and extremely swift, was seen more than once. On July 14, "Mr. Gore, who went out with his gun, had the good fortune to kill one of these animals," adding, "This animal is called by the natives *Kangaroo*." Kangaroos, however, had been seen by earlier travellers, and these may even be the animals referred to by Dampier when he tells us that on the 12th of August, 1699, "two or three of my seamen saw creatures not unlike wolves, but so lean that they looked like mere skeletons."

The whole animal population of the globe is termed the Animal Kingdom, in contrast with the world of plants, or Vegetable Kingdom. The highest sub-kingdom of this is that of the Vertebrata, of which the Mammalia form the highest class, to which class the Kangaroos belong. Of these animals there are many species arranged in some four genera; the true Kangaroos forming a genus, *Macropus*, which is very nearly allied to three others, namely, *Dorcopsis*, with a very large first grinding tooth; *Dendrolagus* (Tree Kangaroo), which frequents the branches of trees, and has the fore limbs but little shorter than the hind; and *Hypsiprymnus* (Rat Kangaroo), which has the first upper grinder compressed and vertically grooved. The species all inhabit Australia and the adjacent islands. They all agree in having the second and third toes slender and united in a common fold of skin; the hind limbs longer than the fore limbs; no inner metatarsal bone; all the fore toes provided with claws; and six upper together with two lower incisors. These five characters coexist in no other animal.

The family Macropodidæ is one of six which, together with it, make up the larger Kangaroo Order, the exact relations of which necessitate a cursory view of the others being taken. The Bandicoot plainly differs from the Kangaroo in external appearance, but resembles it in having the hind limbs longer than the fore, and also in the structure of the hind feet, which are similarly modified, but to a less degree, a rudimentary inner toe being present. It is an example of the family Peramelidæ, one member of which, *Chœropus*, is very exceptional, in that the hind toes, except the fourth, are exceedingly reduced and functionless, at the same time that its anterior digits are only two in number. The Phalanger is a type of the Phalangistidæ, arboreal, nocturnal animals, in which the limbs are of nearly equal length, with the second and third hind toes united, and a large opposable thumb. Some have prehensile tails, others expansions of the skin in the flanks to act as a parachute in leaping. The Koala (*Phascolarctus*) and *Tarsipes* are aberrant members; the former without a tail, the latter with minute and few teeth. The genus *Cuscus* is found in New Guinea and Timor. The Wombat (*Phascolomys*) forms a distinct family. It is a burrowing, nocturnal animal, the size of a badger, with a rudimentary tail, as well as peculiar feet and rodent-like teeth.

The Dasyuridæ, or family of the native cat, wolf, and

devil, are so called from their predatory and fierce nature. They have large canine teeth and sharp molars. The second and third toes are no longer bound together, whilst the great toe is absent or small. Myrmecobius is a peculiar genus, remarkable for the great number of its back teeth. The Tasmanian Wolf is confined to that island, and will very probably soon become quite extinct, because of its destructiveness to the sheep of the colonies. It differs from all other members of the Kangaroo order in that cartilages represent the marsupial bones found in every other member of the order. The last family consists of the true Opossums, which differ from all above referred to in inhabiting America only, not Australia. They are called Didelphidæ; one species is aquatic in habit, and web-footed.

Such are the very varied forms composing the six families which together make up the Kangaroo order. What is its relation to those of the other Mammalia? Very noticeable in it is the very great diversity of form, dentition, and habit found in the order, some being arboreal and vegetarian, others terrestrial and carnivorous, &c.; nevertheless, these so varied marsupial forms possess in common important characters by which they differ from all other mammals. These characters, however, relate mainly to the structure of their reproductive organs, as to the great importance of which characters naturalists are agreed. The angle of the lower jaw is also peculiar. Almost every mammal which has marsupial bones has the angle of its jaw inflected, or else has no angle at all, whilst every animal which has both marsupial bones and an inflected jaw-angle, possesses also those other special characters which distinguish the marsupials from all other mammals. We have, therefore, at least two great groups, one non-marsupial, containing man, the apes, bats, cats, hoofed beasts, &c.—the Monodelphia; the other containing the marsupials only—the Didelphia. There is a third group containing only the Ornithorhynchus and Echidna, which form by themselves alone a third group, Ornithodelphia.

As to its zoological relations, we may therefore say that the Kangaroo is a peculiarly modified form of a most varied order of Mammalia (the marsupials), which differs from all ordinary beasts (and from man) by very important anatomical and physiological characters, the sign of the existence of which is the coexistence in it of marsupial bones with an inflected angle of the lower jaw. As to the geographical relations of the Kangaroo, a study of their distribution over the world shows that the Kangaroo is one of an order of animals confined to the Australian region and America, the great bulk of the order, including all the Macropodidæ, being strictly confined to the Australian region.

The lecturer concluded by explaining the geological relations of the Kangaroo and its order, pointing out that in Australia we have an instance of zoological "survival" connecting the existing creation with the triassic period.

*MAGNETO-ELECTRIC MACHINES**

II.

IN 1871 M. Jamin communicated to the French Academy of Sciences a short note by M. Gramme, on a magneto-electric machine which gave electrical currents always in the same direction by the revolution of an electro-magnetic ring between the poles of a permanent magnet. The construction of the electro-magnetic or ring armature in Gramme's machine differs in some mechanical details from that of the transversal electro-magnet of Pacinotti, and the serious mistake of applying the rubbers which carry off the current at the wrong place is avoided. We must therefore regard the Gramme machine as the first effective magneto-electric machine constructed to give continuous currents all flowing in the same direction. Before entering into the details of its construction it may be useful, even at the risk of some repetition, to describe as briefly as possible the principles on which the action of the electro-magnet or ring armature depends.

In its simplest form this armature consists of a ring of soft iron, round which is wound a single closed coil of copper wire or other metallic riband, covered with silk, except at a single point in each loop of the coil, which is left exposed in order to make contacts. In Fig. 4 such a ring is shown, placed between the poles of a permanent magnet. The parts of this ring contiguous to the poles N S of the fixed magnet will acquire respectively polarity of the opposite kind to that of the neighbouring pole, while the parts of the ring O O′, at the end of a diameter

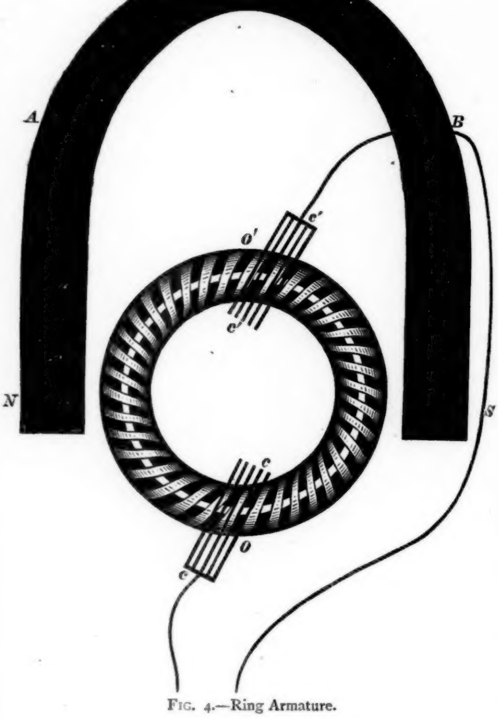

FIG. 4.—Ring Armature.

at right angles to the line joining the poles, will be neutral. If the ring is made of homogeneous metal, this statement will be strictly exact so long as it is at rest, but if it be made to revolve rapidly on an axis perpendicular to the plane of the fixed magnet, the poles of the ring, as well as the neutral points, will be slightly displaced, as M. Gaugain has shown, in the direction of the motion. This arises from what is called the coercive power of iron; that is, from the circumstance that even the purest iron will not acquire or lose magnetism in an inappreciably short period of time. The change in the distribution of the magnetism in the ring from this cause is, however, inconsiderable, and may easily be allowed for.

To make the explanation clearer, let us suppose that there is only one loop of wire, *a* (Fig. 5), upon the ring, and that this loop is moveable and in connection with a galvanometer *g*. If now the loop is moved along the ring (assumed to be at rest) from the neutral line O towards S′, a current will be developed in a certain direc-

* The substance of a Lecture, with additions, delivered at the Belfast Philosophical Society, March 17, by Dr. Andrews, F.R.S., L. & E. (Continued from p. 92.)

tion, the intensity of which will increase till the loop reaches S′, after which the current, always preserving the same direction, will diminish till the loop arrives at O′, when the current will for a moment fall to zero, to be succeeded by a current in the opposite direction as the loop leaves O′. This current will in like manner increase during the advance of the loop to N′, when it will attain a maximum, and afterwards diminish till it arrives at O, where, after passing through zero, the direction will again change. There will thus be a current always flowing in one direction as the loop moves from O through S′ to O′, and in an opposite direction as it moves from O′ through N′ to O. Now if the loop, instead of being moveable upon the ring, be firmly attached to it, and the ring itself carrying the loop be rotated on its axis in the plane of the fixed magnet N M S, it will be found that the currents developed will correspond both in direction and

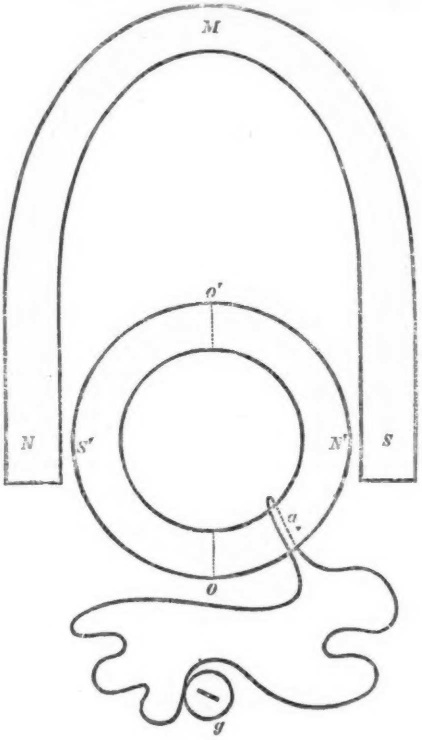

FIG. 5.

intensity with those produced in the moveable loop, provided we allow for the small displacement in the position of the poles of the ring arising from its motion.

The foregoing statement may be extended from a single loop to any number of loops forming part of a coil extending over the whole of the iron ring (Fig. 4). Each loop of such a coil, during one-half of every revolution, will tend to give a current in one direction, and during the other half, a current in the opposite direction, and the electromotive force thus produced will augment with the number of loops in the coil. If, then, metallic conductors, *c c*, *c′ c′*, are applied to the loops (whose surfaces must be exposed at one point for this purpose) as they pass through the positions O and O′, continuous currents, all in the same direction, will be obtained on rotating the ring without the use of a commutator, unless we apply that term (as Pacinotti has done) to the system of conductors or rheophori by which the currents are carried off.

In order to obtain currents of high intensity, the single coil must be replaced, as in similar machines, by a number of coils of thin wire rolled one above the other and carefully insulated. To carry off the current, these coils must be divided into separate helices, with the adjacent terminals of the wires of the helices in metallic connection, so that the iron ring may be always surrounded by an endless conductor of great length. I have already described the arrangements adopted in the transversal electro-magnet of Pacinotti. The construction of the ring armature in Gramme's machine will be readily understood from Fig. 6, in which it is represented in different stages of its construction, so as to show the manner in which the principal parts are connected.* At A a section of the iron ring itself is shown, composed of a bundle of iron wires; at B B the helices, or bobbins, are seen both in section and detached; and at R R the form is shown of one of the insulated copper conductors, to which the contiguous ends of the wires of the helices are attached, and from which the current is drawn off by means of rubbers or brushes formed of flexible bundles of copper wire. These brushes are so applied at the neutral positions of the ring that they begin to touch one of the conductors R, before they have left the preceding one. In this way no actual break or interruption occurs in the current. The permanent magnets employed in the smaller

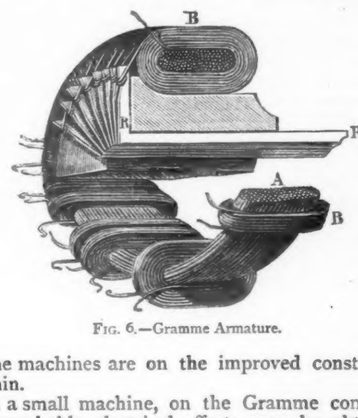

FIG. 6.—Gramme Armature.

Gramme machines are on the improved construction of M. Jamin.

With a small machine, on the Gramme construction, very remarkable electrical effects may be obtained. I will give the results of a few experiments which I recently made with one of the two machines exhibited at the late meeting of the British Association, and which are now in Queen's College, Belfast. This machine was able to heat to full ignition in daylight a platinum wire one foot in length, and weighing 12 grains. With a voltameter formed of two slips of platinum foil, exposing each a surface of 1·25 square inches, and at the distance of half an inch from each other, immersed in dilute sulphuric acid, water was freely decomposed. For 100 turns of the machine, the volumes of the mixed gases collected at different rates of turning were as follows:—

In	34	seconds	... ...	2·60 cubic inches
„	45	„	... ...	2·53 „
„	75	„	... ...	1·45 „
„	135	„	... ...	0·35 „

From these observations it appears that, under the conditions of this experiment, the quantity of water decomposed for the same number of revolutions of the ring increases quickly with the rate of the motion till a certain

* I take this opportunity of expressing my obligations to M. A. Niaudet-Breguet for his kindness in enabling me to give the admirable figures of the Gramme Machine which illustrate this paper. They first appeared in a short work on the Gramme Machine, recently published by M. Breguet, to which I beg to refer for more detailed information regarding its practical applications ("Machines Magnéto-électriques Gramme." Par M. A. Niaudet-Breguet. Paris, 1875).

rapidity is attained, after which little further change occurs.

An interesting experiment may be made with these machines, which illustrates a well-known dynamical principle, by turning the machine at a steady rate, with the wires for transmitting the current disconnected, and observing the great additional force required to maintain the motion on connecting the wires.

The machine may be converted into an electro-magnetic one by transmitting the current from a voltaic pile through the helices of the iron ring, which will then rotate upon its axis. If the current be supplied by another magneto-electric machine, the same result will be produced, and we shall thus have mechanical force, after assuming the form of current electricity, reappearing, but with some loss, in the form of mechanical force. In an experiment on the large scale described by M. Breguet, the loss amounted only to thirty per cent. If during this experiment the machine which supplies the current has its motion reversed, the other machine will soon come to rest, and afterwards begin to turn in the opposite direction. The intensity of the current, M. Breguet remarks, augments with the velocity of the rotation, the electromotive force having been proved by experiment to be proportional to the velocity. At first view it might appear that the resistance would remain constant; but as the intensity is found not to be proportional to the velocity of an invariable circuit, we are led to the conclusion that the resistance of the machine is not constant. This important point has been established by M. Sabine, but the details of his experiments have not been published. The increase of resistance is, however, so small, that a machine which gives with a velocity of 100 turns per minute a current equal to that of one small Bunsen's element, will give with a double velocity a current equal to two such elements a little larger, and with a quadruple velocity a current equal to four still larger elements of Bunsen. It is certain that this increase of electromotive force cannot be indefinite, but must tend towards a limit; but this limit does not appear to have been reached even with a velocity of 3,000 turns per minute.

(*To be continued.*)

ON THE TEMPERATURE OF THE HUMAN BODY DURING MOUNTAIN-CLIMBING

IN the year 1869 both Dr. Wm. Marcet, of Nice,* and Dr. Lortet, of Lyons,† published the results of thermometric experiments prosecuted by themselves on themselves during the ascent of Mont Blanc. Both physiologists discovered that during the act of ascent, if it were rapid and prolonged for any considerable time, the temperature of the body fell considerably, as much as 3·6° F. in the case of the English, and even 8·6° F. of the French observer. The temperature was taken in the mouth, and read off by means of a small reflector attached to the thermometer, which is a much more satisfactory manner of recording reducing temperatures than the employment of maximum registering instruments. Dr. Marcet tells us that in order to assure himself that the cooling of the body during the ascent was really due to the muscular effort, and not to the effect of the rarefaction of the air, he made one ascent (from Cormayeur to the plateau of Mont-Frety, about 2,440 yards high) partly on mule-back. After having gone two-thirds the distance, his temperature was 97·5° F., when, leaving the mule, he performed the rest of the journey on foot as quickly as possible. Just before arriving at the end, his temperature was not above 95° F., or 2·5° below what it was thirty-five minutes before, at the lower level. Another peculiarity observed by this author is that the body-temperature, after having diminished during an ascending walk, rapidly rose again upon rest being taken, or on the speed being reduced.

* "Archives des Sciences Physiques et Naturelles." 5e serie, t. xxxvi. p. 247. (Geneva.

† "Recherches Physiologiques sur le Climat des Montagnes" (Paris.)

All these unexpected results have, from the absence of fresh facts to throw light upon them, been but little discussed. It has been asked whether the above-described fall of temperature depends on the transformation of the energy of muscular action into work instead of, as usual, into heat in the body. The answer to this question is, however, not so easy as it might at first sight appear. If the exalted temperature of warm-blooded animals in a state of rest is the index of the amount of internal work done by the heart and the respiratory muscles, then extra muscular work will produce a proportionately greater rise of body-temperature, as it is employed in doing less external work, and the reverse; from which consideration it is rendered theoretically probable that the rise in temperature attending a rapid ascent of an incline would be much less considerable than that accompanying a similar effort which is attended by no external effect. In fact, the temperature of an individual in the act of throwing oranges forcibly away in all directions should be scarcely above the normal, whilst if he continually throws one up, again catching it, his temperature should rise considerably. In the one case the muscular effort is employed in heating the ground against which the moving oranges come in contact whilst being brought to rest; in the other case the energy lost to the body in the upward projection of the mass is regained in the form of heat when the muscles of the limbs resist its downward movement in catching it.

At this stage of the inquiry the elaborate investigations of Prof. Forel, of Lausanne,* prosecuted with indefatigable industry during the last four years, form an important addition to the literature of the subject. This physiologist, in a most painstaking and thorough manner, has investigated the whole problem, together with all the minor details associated with it: the results he has arrived at have consequently a wider interest than the simple solution of the question which originally led to their being commenced.

In his earlier series of experiments, Dr. Forel, whilst staying at the Rhone Glacier, at Zermatt and at the Lake of Geneva, ascended the Grimsel, the Riffel, and to Chigney, as well as to other neighbouring heights, in the end arriving at the following conclusions:—firstly, that the method of measuring the body-temperature in the mouth is not sufficiently precise for the study of the influence of muscular exercise on the general temperature of the body; and, secondly, that the act of ascending normally produced an *elevation* of the temperature of the body to the extent of several tenths of a degree, which diminishes during the subsequent repose, in tending to regain the normal standard.

These results, obtained in 1871, being directly at variance with those of Doctors Marcet and Lortet, Dr. Forel repeated his experiments with greater precision during the years 1873 and 1874. He commenced by determining the relative values of the different regions of the body in which it is possible to employ the thermometer for the estimation of the general temperature. More than a hundred observations in the floor of the mouth led him to reject that position for the thermometer, chiefly because it is next to impossible, during muscular exercise, to retain the mouth closed for any considerable time in a cold, dry, rarefied air. The palm of the hand, the arm-pit, and the external auditory meatus were rejected as being even less advantageous. The rectum was the last resource, and its advantages were found to be so great that all the most important results, to be mentioned directly, were arrived at from temperatures obtained in that situation.

The author commenced by forming a curve which repre-

* "Expériences sur la Température du corps Humain dans l'acte de l'ascension sur les Montagnes." (Geneva and Bale, 1871 and 1874.)

sents the average temperature of his own body at the different hours of the day, in order that he might eliminate this factor as a disturbing cause in his special observations. The curve represents an elevation of the temperature between the hours of 3 and 9 A.M., and a fall between 9 P.M. and 2 A.M., with an elevated temperature during the day, the undulations of which are far from constant and are difficult to characterise. In employing these results practically, Dr. Forel has introduced a method of turning them to account, which is as useful as it is precise and ingenious. In any special experiment, calling t the temperature, and T the normal temperature at the time of observation as found from the tabulated curve, then

$$t - T = t'$$

t' being the difference between the observed temperature and that which, under ordinary circumstances, it would be, either above or below it. As examples, we will take two given by the author :—

At 12 o'clock, noon, $T = 99{\cdot}09°$ F. On one particular occasion t was found to be $99{\cdot}5°$ F., and therefore

$$t - T = t' = + 0{\cdot}41° \text{ F.}$$

On a second occasion, at the same time of day, the temperature observed was $98{\cdot}78°$ F., from which it is evident that

$$t' = - 0{\cdot}31.$$

By the employment of this very simple means, therefore, the complications dependent on the time of day at which an observation is made may be immediately eliminated; all comparisons being between the different values of t', and not of t. Whether the assumption that the daily curve of body-temperature-change depends on the time of the day at which the observation is made, and on the time only, is a question into which the author does not enter, notwithstanding that such is the case has been by no means proved.

Turning now to the results arrived at from the investigation, the position in which the subject was left by Marcet and Lortet may be thus summarised :—

1. The temperature of the body, as a rule, falls during the act of ascending an incline.

2. During the time of the "mountain sickness," which so frequently accompanies the ascent of lofty heights, the body-temperature falls in a very marked manner.

Dr. Forel's earlier experiments, conducted in 1871, in which the thermometer was retained in the mouth, as was done by Marcet and Lortet, being directly opposite in their tendency, led him to commence the whole subject in 1872, as he remarks *ab ovo*, under his improved conditions.

As to the effect of an uncomplicated ascent, two instances are given in full, in both of which a considerable *rise* in temperature accompanied a rapid ascent of about an hour's duration. In one of these, at the end of the journey, the thermometer registered 102·5° F., whereas it was slightly below 100° F. on starting.

In a second series, three illustrations are given of the effect of well-marked fatigue, just short of exhaustion. The following are the deductions drawn from them :—

1. Even in conditions of great fatigue, the human body rises in temperature upon the muscular effort of ascending a height.

2. It is impossible for the author to determine if the elevation of animal heat due to the movement of ascension diminishes in proportion to the increase of the muscular fatigue.

Next as to the influence of an empty stomach on the temperature curve; and it must be noted, with regard to this point, that both Marcet and Lortet have stated that the fall in temperature accompanying an ascent is more marked during a fast than shortly after a meal. On himself, Dr. Forel, however, again proves that a fast of twelve or even twenty-four hours is no obstacle whatever to the rise of temperature which attends the muscular effort of ascending a hill.

By collecting and comparing the temperature-curves produced in ascending and descending inclines, the author is enabled to verify the theoretical necessity that the body-temperature is raised more by a descent than by an ascent. From twenty-one experiments, the average rise in temperature attending the act of ascending is found to be 2·412° F., whereas the mean of seven descents is found to be 2·772° F. The difference, 0·36° F., is small, it is true. If this fact is reliable, we find that a certain amount of heat is transformed into mechanical work during the act of ascent, a certain quantity being returned to the organism from without, under the opposite condition.

There are several minor points which Dr. Forel discusses in a particularly instructive manner, amongst which are the time of cooling after muscular exertion, the effect on the pulse and respiration of mountain climbing, and the cause of mountain sickness. He terminates his very interesting observations by the account of an ascent of Mont Rosa in July 1873 (15,217 feet), in which, notwithstanding that he suffered from mountain sickness, the body-temperature never showed any tendency to fall throughout, and was 101·5° F. on his reaching the highest point.

From this summary of Dr. Forel's results, when taken in connection with those of Dr. Allbutt,* it is evident that the temperature-fall observed by Drs. Marcet and Lortet during mountain climbing requires re-verification, and cannot be accepted as a physiological fact until a fallacy has been shown to exist in the method of investigation adopted by the Swiss experimenter. A. H. G.

NOTES

At Cairo, on the 2nd inst., the inaugural meeting took place of the Société Khédivale de Geographie, under the presidency of the eminent traveller Dr. Schweinfurth and the patronage of H.H. the Khedive, who has shown special favour to the young society, having placed at its disposal a handsome suite of apartments furnished in suitable style, and also presented a valuable library, besides subscribing 400*l*. a year to the funds. This cannot but be gratifying to the friends of science and progress, and is a hopeful sign for the future of Egypt and of the extensive region from which it claims allegiance. Let us hope that like results will follow the intercourse between this country and the Sovereign of Zanzibar. With these two African potentates on the side of progress, the advantages to knowledge, as well as to Africa, could not but be great. At all events, under the powerful patronage of the Khedive, this Egyptian Geographical Society is bound to make valuable contributions to our knowledge of North Africa. Dr. Schweinfurth, in his inaugural address, which was characterised by great fervour, spoke of the domain and progress of geography. "It has become," he said, "an immense domain, the meeting-place of all branches of human science. The geography of the present does not aim at merely describing the external form of the earth, the vesture which it has assumed; it seeks to show the chain of hidden causes of which this form is the expression." He then spoke of Africa and the great interest attaching to it, and especially to the Nile, the sources of which he believes contain the key to all the mysteries of Africa. Dr. Schweinfurth then referred to the history of Egypt and its progress under its present ruler, by whose special desire the Society has been organised. The motto of the Society, he said, should be *Nusquam otiosus*, and its duty *Centraliser et encourager*. After pointing out to those who take a "utilitarian" view of science, that all the comforts and commodities of modern life are due to researches which, though purely theoretical in their origin, have yielded magnificent practical results, Dr. Schweinfurth indicated the benefits to be gained from the increase of geographical know-

* *Journal of Anatomy and Physiology*, vol. xi. p. 106.

ledge, and described the organisation of the new Society and the task which lies before it. He showed what advantages a Society so situated had over European societies for extending our knowledge of Africa, and pointed out what yet remained to be done ere the topography of North Africa could be considered anything like completely known. We notice that the principal geographical societies of Europe and America have sent their congratulations to Dr. Schweinfurth on the founding of this Society; England's name, however, is not mentioned.

MR. MACLEAY, who has organised the expedition to New Guinea, our readers may remember, has already liberally endowed Sydney University. The ship he has fitted out for exploring New Guinea is a 400-ton man-of-war. His chief object is to enrich his Natural History collection, and he intends to do deep-sea dredging; he takes also a steam launch for ascending the rivers. There is one immense river, named the "Fly" River, after H.M.S. *Fly*, about which nothing is known. Mr. Macleay thinks that he will be able to ascend some 200 miles.

M. POLJAKOW, commissioned by the Russian Geographical Society, undertook a journey last year into the region of the Upper Volga, chiefly for zoological purposes, though he also obtained some important geologico-geographical results, an account of which appears in Heft vi. of Petermann's *Mittheilungen*. From the observations which he made, Poljakow concludes that the Scandinavian Finlandic glacier which once held in its fetters the government of Olonez and the neighbouring governments, must have stretched far into the basin of the Volga and over the boundaries of the Waldai plateau; and that by the unequal levels of the lakes formed by the melting of the glacier, the slender remains of which are seen in the existing lakes, undoubtedly a connection existed between the basin of the Volga and the Arctic and Baltic seas. Judging from the fauna, Poljakow concludes that the present upper course of the Volga must have been joined to the middle and lower course at a recent period and in a manner accidentally. The upper river has an entirely different and indeed a more northern water fauna than the middle and lower river. In this respect is the Scheksna to be considered the natural upper part of the Volga, for it contains the very same fishes as that river as far as Bjelosero.

DR. FOREL, of Lausanne, has for several years been investigating what are known as the *Seiches* of the Lake of Geneva. *Seiche* is applied locally to certain oscillatory movements which are occasionally seen to occur on the surface of the lake. The phenomenon had been investigated by previous observers, among others by Saussure and Vaucher, who attributed the phenomenon to variations in atmospheric pressure; in this, Forel, who has most minutely investigated the phenomenon, agrees with them. The phenomenon is found to occur on other Swiss lakes, and Forel believes it will be found in all large bodies of water. Indeed, he recognises in the *Seiche* probably the most considerable and the grandest oscillatory movement which can be studied on the surface of the globe. His investigations have led him to the conclusion that the *Seiche* on the Swiss lakes is an oscillatory undulation (*ondulation de balancement*), having a true rhythm, and that the phenomenon is not occasional, but constant, though varying in degree. The duration of a *Seiche* is a function of the length and depth of the section of the lake along which it oscillates; this duration increases directly with the length and inversely with the depth of the lake. The instrument he has devised for the investigation of the phenomenon he calls a *plémyramètre* ("tide-measurer"). A detailed account of Forel's investigations will be found in two papers in the *Bull. de la Soc. vaud. des Sciences Naturelles*, tomes xii. and xiii. Both papers have been republished separately.

HEFT VI. of Petermann's *Mittheilungen* contains a valuable paper by Vice-Admiral B. v. Wüllerstorf-Urbair on the Meteorological Observations made by the recent Austro-Hungarian Arctic Expedition, with an analysis of the ship's course. The paper is accompanied by a chart showing the drift of the ice, the course of the ship, the depths of soundings, the direction of the wind, and various other data.

AT the meeting of the Geographical Society on Monday, a lecture was delivered by Admiral Sir Leopold M'Clintock on "Arctic Sledge Travelling." After an account of the expeditions of former Arctic travellers, from Parry downwards, Sir Leopold gave a description of the appliances required for Arctic travelling, and of the difficulties to be encountered. To sledging, he said, we are indebted for almost all our Arctic experiences, and to sledging we shall owe the principal share of whatever work may be done by the brave men now going out. The greatest bar to their progress would be ice too thin to sledge over; sledge-bearing ice or open water their equipments will enable them to traverse.

AN opportunity will occur of sending letters for the Arctic ships *Alert* and *Discovery* by the exploring yacht *Pandora*, which will leave Portsmouth about the 23rd instant, Mr. Allen Young, commanding that vessel, having consented to receive letters, newspapers, &c., upon the chance of their being delivered to or deposited for those ships. No articles of value should be sent, and letters, &c., should be addressed to the General Post Office, and marked "Per exploring yacht *Pandora*."

A VERY full and interesting *résumé* of the progress of geographical discovery and of the sciences connected with geography, by M. Charles Maunoir, appears in the April number, just issued, of the *Bulletin* of the French Geographical Society; it is illustrated by a series of small maps. The same number contains the plan of a scientific journey into the interior of Indo-China, by Dr. J. Harmand.

NEW YORK telegrams of June 12 report a terrible earthquake in the Cucuta Valley, Republic of New Grenada. Cucuta, it is stated, has been entirely destroyed. Five other towns were nearly destroyed, and 16,000 persons are reported lost, out of a population of 35,000.

A TELEGRAM dated Barcelona, June 10, states that some shocks of earthquake had been felt there and in the neighbouring villages.

THE U.S. Hydrographic Office, of which Commodore R. H. Wyman, U.S.N., is superintendent, has commenced the systematic establishment of secondary meridians by telegraphic exchange of time-signals. Lieut.-Commander F. M. Green, U.S.N., is at present in charge of the work, and has during the past winter made observations at Panama, Colon, Kingston, Santiago di Cuba, and Havana. The starting-point used for the determination of longitude has been the meridian of Key West, Florida, established with great care by the U.S. Coast Survey. In addition to longitude observations, the latitude of each station has been determined with the zenith telescope. The work will be continued next winter through the Windward Islands to Guiana and Brazil. The liberal conduct of the companies owning the cables has much facilitated the successful prosecution of the work.

WE may see from the following extract from the New York *Nation* how very closely our doings on this side of the water are watched. The appointments referred to we have already announced in NATURE, but the comments upon them by the *Nation* indicate what we hope will be the method pursued by England in the course of time, though we fear the course will be a very long one. "Two recent appointments," the *Nation*

says, "in the University of Zürich seem to merit notice, as signs of the times. One is that of Prof. W. Wundt to the Chair of Philosophy, the other that of Prof. E. Hitzig to the Chair of Psychology. Wundt has long been occupied at Heidelberg, first as Assistant, then as 'Ordinary' Professor of Physiology, whilst Hitzig has been a medical practitioner and lecturer on electro-therapeutics in Berlin. So far as we know, the latter has written nothing on purely mental science. His discovery of the irritability of the surface of the brain is his chief title to fame; all that he has written shows erudition, great experimental thoroughness, and conscientiousness in drawing inferences. Wundt is one of the most learned of German investigators. His own special work has lain most in the line of the senses and the nervous system, the territory common to mind and matter; and all the elements of his training hitherto unite to make him an eminently well-qualified teacher of mental science. Indeed, we doubt not that his long apprenticeship in physiology was accepted by him merely that he might be the better educated for philosophy. In this country such appointments would probably provoke a good deal of orthodox alarm. But in Germany not only is thought more fearless of consequences, but 'camps' in opinion are much less clearly defined, and materialistic and spiritualistic tendencies keep house together most amicably in the same professional brains. We cannot help regarding such appointments as these as hopeful tokens of a new era in philosophical studies—an era in which the old jealousy between the subjective and the objective methods shall have disappeared, and in which it shall be admitted that the only hope of reaching *general* truths that all may accept is through the co-operation of all in the minute investigation of special mental processes. We may then see solid philosophical conclusions gradually emerging from the mass of discoveries of detail, just as happens in the sciences more especially recognised as 'inductive.'"

WE take the following from the *Athenæum*:—Mr. William Davis, who has been an attendant at the British Museum since 1843, but has practically fulfilled, for a long time past, duties requiring considerable scientific acquirements for a salary which, after the lapse of thirty years, had risen to the magnificent sum of some twenty-five shillings a week, was on Wednesday appointed by the Trustees an assistant in the Department of Geology. Mr. Davis was the first recipient of the Murchison Medal of the Geological Society, and is a well-known authority upon vertebrate fossils, especially fishes and mammalia.

THE series of papers on Portuguese Travel by Mr. John Latouche, which have appeared in the *New Quarterly Magazine*, are shortly to be published by Messrs. Ward, Lock, and Tyler, under the title of "Travels in Portugal," with illustrations by the Right Hon. T. Sotheron-Estcourt.

A TELEGRAM, dated "Strangway Springs, April 17," has been received from Mr. Ernest Giles, who has been exploring to the north of Fowler's Bay, Australia. He had had one long stretch of 220 miles without water; all the horses died, and he was only saved by his two camels. Mr. Lewis's expedition to Lake Hope, South Australia, has proved successful. Lake Hope he found perfectly dry. Before completing his work, Mr. Lewis purposes endeavouring to discover a route between the south-west portion of Queensland and the north-west of New South Wales, with a view of establishing direct overland communication with the former colony.

THE annual meeting of the Palestine Exploration Fund was held last Thursday. Since the Society was founded in 1865, four expeditions have been made, and surveys and excavations effected. The surveys have extended from Mount Carmel in the north to Beersheba in the south, and from Askelon in the west to the Dead Sea.

THE death of the lamented Rémusat has created a majority in favour of M. Dumas in the election which will take place at the Académie Française five months hence. It was owing to the prospect of a vacancy that the election was postponed when the Academicians were unable to agree after three successive meetings.

THE death is announced, on June 9, at the age of seventy-nine years, of M. Deshayes, Professor in the Paris Musum of Natural History.

La Revue Scientifique records the death, on May 11, at the age of thirty-two years, at Algiers, of a distinguished Mussulman chemist, Abdallah ben Mohammed. His mission was to instruct in the physical sciences, and especially in chemistry, the native Algerians; for this purpose he had to devise an Arabic terminology.

THE death is announced of Senhor Joaquim Henriques Fradesso da Silveira, director of the Meteorological Observatory of the Infanta Don Louis at Lisbon.

THE Pr ofessorship of Chemistry at Munich, we learn from the *British Medical Journal*, which has remained vacant since the death of Liebig, has been accepted by Prof. Baeyer of Strasburg, who will commence his duties next winter session.

THE jury of the Exhibition of the French Central Society of Horticulture has awarded a large gold medal to M. De la Bastie for his discovery of hardened glass, on account of the services it is likely to render to horticulture.

THE Annual Report of the United States Geological and Geographical Survey, describing the explorations of the year 1873, which has just reached us, contains, besides the descriptive letter-press, several valuable illustrations of some of the more recently discovered genera and species of Mammalia belonging to genera closely allied to Dinoceras (Marsh). These include *Symborodon bucco* (Cope), *S. Saltirostris*, and *S. ater*, all very peculiar forms.

WE have received the third Annual Report of the Zoological Society of Philadelphia, just published, which tells very strongly in favour of the institution. The additions by presentation and purchase are numerous, including six Giraffes, an Elk, an African and an Indian Elephant, and a Ka-Ka Parrot. We may judge that the Gardens are constantly kept in view by the citizens in their travels, from the fact that not less than twenty-three alligators were presented within three months.

THE President of the Italian Geographical Society has received favourable intelligence of the expedition sent to examine the possibility of conducting the waters of the sea into the hollow basins of the Sahara. The expedition will be divided into two parties at Gares. One is to explore the Oasis of Gerid, and carry out some interesting collateral researches among the ruins of Carthage, particularly the remains of the aqueducts and the remarkable lead mountain of Gebel Drucas.

AN attempt which has just been made to introduce living humming-birds into the Paris Jardin d'Acclimatation has failed, although a traveller managed to bring six alive to Paris by feeding them with honey. The only other humming-birds which have reached Europe alive were those brought by M. Delattre in 1855 from Central America, but these died a fortnight after their arrival in Paris.

"NURAGGHI SARDI, and other Non-historic Stone Structures of the Mediterranean Basin," is the title of an illustrated pamphlet by Capt. S. P. Oliver, who offers it "as a slight contribution towards the constantly increasing knowledge of those pre-historic remains which are scattered in mysterious groups throughout the Old World." Carson Brothers, of Dublin, are the publishers.

THE additions to the Zoological Society's Gardens during the past week include a Grant's Gazelle (*Gazella granti*) from East Africa, presented by Dr. Kirk; a Beccari's Cassowary (*Casuarius beccarii*) from New Guinea, presented by Sir James Fergusson; an Owen's Apteryx (*Apteryx oweni*), two Weka Rails (*Ocydromus australis*), a Black Wood Hen (*Ocydromus fuscus*), from New Zealand, presented by Dr. G. Hector; two Australian Cranes (*Grus australasiana*) from Australia, presented by the Acclimatisation Society of Wellington, New Zealand; a Brown Indian Antelope (*Tetraceros subquadricornutus*) from India, a Dufresne's Amazon (*Chrysotis dufresniana*) from South-east Brazil, four Vulturine Guinea Fowls (*Numida vulturina*) from East Africa, an Anaconda (*Eunectes murinus*) from South America, purchased.

RECENT PROGRESS IN OUR KNOWLEDGE OF THE CILIATE INFUSORIA *

I BELIEVE that the object contemplated by the addresses which it has been the custom of your Presidents to deliver year after year to the Fellows of the Linnean Society will be best fulfilled by making them as much as possible the exponent of recent progress in biological science. The admirable addresses with which my distinguished predecessor has during his long tenure of office so greatly enriched our journal, afford an example as regards the exposition of botanical research which may well be followed in biology generally. The field, however, which thus offers itself is so wide, the activity in almost every department so intense, that the necessity of restricting the exposition within a limited area becomes imperative if it be expected to produce anything like a definite picture instead of a vast assemblage of images confused and ill-defined by their very multiplicity and by the condensation which would be inseparable from their treatment.

While thus imposing on myself these necessary limits, it is almost at random that I have chosen for this year's address some account of the progress which has recently been made in our knowledge of the CILIATE INFUSORIA—a group of organisms whose very low position in the animal kingdom in no way lessens their interest for the philosophic biologist, or their significance in relation to general morphological laws.

To enable you to form a correct estimate of the value of recent researches, it may be well to bring before you in the first place, as shortly as possible, the chief steps which have led up to the present stand-point of our knowledge of these organisms.

It is scarcely necessary to remind you that the first important advance which during the present century was made in our knowledge of the Infusoria dates from the publication of the great work of Ehrenberg,* whose unrivalled industry opened up a new field of research when, by his expressive figures and well-constructed diagnoses, he made us acquainted with the external forms of whole hosts of microscopic organisms of which we had been hitherto entirely ignorant, or which were known only by such figures and descriptions as the earlier observers with their very imperfect microscopes were able to give us.

Ehrenberg, however, as we all know, did not content himself with pourtraying the external forms of the microscopic organisms to whose study he had devoted himself, but sought also to determine their internal structure, of which scarcely anything had been hitherto known. In this direction, no less than in the other, the perseverance of the celebrated microscopist never flagged; but, unfortunately, at the very commencement of his researches he slid into a misleading path, and was never again able to find the right one.

Everyone knows how Ehrenberg, in accordance with preconceived notions of the high organisation of *all* animals, attributed to the Infusoria a complicated structure; how, while he rightly distinguished them from the Rotiferæ with which they had been confounded by previous observers, he yet regarded them as intimately related to these representatives of a totally different type; and how, in attributing to them a complete alimentary canal with numerous gastric offsets, he took this feature as their most important character, and designated them by the name of *Polygastrica*. And it is probably a matter of surprise to many of us, that with the overwhelming mass of evidence which subsequent research has brought to bear against the truth of the polygastric theory, the great Prussian observer should still adhere with undiminished tenacity to his original views.

* Anniversary Address to the Linnean Society, by the President, Dr. G. J. Allman, F.R.S., May 24.
† "Die Infusionsthierchen als vollkommene Organismen." Leipzig, 1838.

Among the authors who, since the publication of the "Infusionsthierchen" have contributed most to a correct estimate of the morphology, physiology, and systematic position of the Infusoria, the names of Von Siebold, Stein, Balbiani, Claparede, and Lachmann, and most recently, Haeckel, stand out conspicuous.

The first who from a strong position offered battle to the authority of Ehrenberg was Carl Theodor von Siebold.* Von Siebold rejected *in toto* the polygastric theory, and, so far from admitting a complexity in the organisation of the Infusoria, he regarded them as realising the conception of almost the very simplest form of life, and attributed to them the morphological value of a cell.

Let us see what is involved in this most significant comparison. The essential conception of a cell is, as you know, that of a more or less spherical mass of protoplasm with or without an external bounding membrane, and with an internal nucleus or differentiated and more or less condensed portion of the protoplasm. It was to a form of this kind that Siebold compared the body of an Infusorium. He called attention to the soft protoplasmic mass of which the body mainly consists; to the external firmer layer by which this is surrounded; and to the variously-shaped body differentiated in the protoplasm, to which Ehrenberg had gratuitously attributed the function of a male generative organ. Here then were, according to Siebold, the protoplasm body substance, the bounding membrane, and the nucleus of a true cell.

The morphological value thus attributed to the true Infusoria —under which were included the Flagellatæ—was extended by Siebold to Amœba and its allies, and to the whole assemblage so constituted he assigned the position of a primary group of the animal kingdom to which he gave the name of PROTOZOA, whose essential character was thus that of being unicellular animals. He then divided his Protozoa into those which had the faculty of emitting pseudopodial prolongations of their protoplasm (Amœba, &c.), and those in which the place of the pseudopodia was taken by vibratile cilia or by lash-like appendages. To the former he gave the name of *Rhizopoda*; to the latter he restricted that of *Infusoria*; and lastly he divided the Infusoria into the mouth-bearing, *Stomatoda* (Ciliata), and the mouthless, *Astomata* (Flagellata). From every point of view Von Siebold's conception of the morphology of the Protozoa, and his sketch of their classification, however much this may have been subsequently modified, must be regarded as marking out an epoch in the history of zoology.

Shortly after this the unicellular theory was strongly supported by Kölliker,† and received further confirmation from the researches of Stein,‡ who, however, was unable to accept it to its full extent. With an industry almost equal to that of Ehrenberg, Stein had the advantage of the more philosophic views of organisation which had emanated from the newer schools of biology, and to him we are indebted not only for more accurate views of the structure of the Infusoria, but for the first important contributions to our knowledge of their development; and though the opinion which he at one time entertained, that the true Acinetæ are only stages in the development of the higher Infusoria, has been abandoned by him, he has nevertheless demonstrated the presence in an early period of the development of certain species, of peculiar pseudopodial processes resembling the characteristic capitate appendages of the Acinetæ, an observation of importance in its bearing on the relations of these last to the true Infusoria. No doubt can remain, after Stein's observations, that the Infusoria in their young state have the morphological value of a simple cell, and it is only after their development has become advanced, and that a marked differentiation has begun to manifest itself in this primordial condition, that there can be any difficulty in accepting their absolute unicellularity.

About this time Balbiani drew attention to some very important phenomena in the life history of the Infusoria.§ It had been known even to the early observers that the Infusoria multiplied themselves by a process of spontaneous fission. They had been frequently observed in the act of transverse cleavage, and had also been noticed in what appeared to be a similar cleavage taking place in a longitudinal instead of a transverse direction. Balbiani, however, showed that this apparent longitudinal

* Siebold, "Lehrbuch der vergleichenden Anatomie," 1845.
† Zeitschr. f. Wissens. Zool., 1849.
‡ Stein, "Der Organismus der Infusionsthiere," 1867.
§ Balbiani, "Recherches sur les organes générateurs et la réproduction des Infusoires." *Comptes Rendus*, 1858, p. 383.

cleavage had in many cases an entirely different significance; that it was, in fact, not the cleavage of a single individual, but the conjugation of two distinct ones; and he connected this phenomenon with what he regarded as a true sexual act.

It was then known that besides the nucleus which occupied a conspicuous position in the protoplasmic mass, there existed in many Infusoria another differentiated body similar to the nucleus but smaller, and either in close contact with it or separated from it by a greater or less interval. To this body the ill-chosen name of "nucleolus" had been given. Now, Balbiani's observations led him to believe that under the influence of conjugation this so-called nucleolus underwent a change and developed in its interior a multitude of exceedingly minute filaments or rod-like bodies, to which he attributed the significance of spermatozoa; while at the same time the nucleus became divided into globular masses, which Balbiani regarded as eggs, and in which he believed he could recognise a germinal vesicle and germinal spot. We should thus, according to this interpretation, have in the Infusoria the two essential elements of sexual differentiation, the spermatozoa and the egg.

Stein, though differing from Balbiani in certain details, accepts in its general facts the sexual theory, and maintains the spermatic nature of the rod-like corpuscles to which the nucleolus appears to give rise. But however real may be the phenomena described by Balbiani and by Stein, the correctness of assigning to them a sexual significance may be called in question; and it is certain that subsequent observation has not tended to confirm the hypothesis that we have in the Infusoria true eggs fecundated by true spermatozoa.

Claparede and Lachmann, two able and indefatigable observers fresh from the school of the great anatomist Johan Müller, now entered the field, and their joint labours have given us a great work on the Infusoria.* In this an entirely new view of the morphology of the Infusoria has been introduced. Receding widely from the unicellular theory of Siebold, they approximate towards the views of Ehrenberg in assigning to the Infusoria a comparatively complex structure; but instead of adopting the polygastric theory of the Prussian microscopist, they attribute to the Infusoria a single well-defined gastric cavity occupying the whole of the space limited externally by the outer firm boundary walls of the softer protoplasmic mass; while this mass is regarded by them as nothing more than a sort of chyme by which the gastric cavity is filled. According to this view, the nearest relations of the Infusoria would be found among the zoophytes, and their proper systematic seat would be in the primary group of the Cœlenterata.

Though few zoologists will now be prepared to accept the conclusions of the Genevan naturalists, the cœlenterate relations of the Infusoria has recently found an advocate in Greeff.† In an elaborate memoir on the Vorticellæ, Greeff sees in the very well-marked distinction between the external or cortical layer and the internal soft body-substance, a proof of the views maintained by Claparede and Lachmann; and he considers this position still further confirmed by the presence in *Epistylis flavicans* of numerous oval or piriform, brilliant, well-defined capsules, which are generally distributed in pairs below the outer layer, and which, under the influence of a stimulus, emit a long filament, thus closely resembling the thread-cells so well known as characteristic elements in certain tissues of the Cœlenterata.

It must be here remarked that the presence of similar bodies in the Infusoria, where they have been described under the name of trichocysts, has long been known. Though varying in form, they all possess a more or less close resemblance to the thread-cells of the Cœlenterata. Their presence undoubtedly indicates a step upwards in the differentiation of the organism, but, as we shall presently see, it offers no valid argument against its unicellularity.

In his admirable "Principles of Comparative Anatomy,"‡ Gegenbaur expresses doubts as to the sexual nature of the reproductive phenomena of the Infusoria, and is disposed to regard the so-called embryo-sphere, to which the nucleus gives rise, in the light of a proliferous stolon, from which several zooids are in some cases thrown off. Arguing from the Acineta-like form of the young in the higher Infusoria, as shown by Stein, and comparing the transitory condition of this with the permanent condition of the true Acinetæ, he believes that we are justified in regarding the Acinetæ as the ancestral form from which the proper Infusoria have been derived. He further compares the contractile vesicle and its canals in the Infusoria with the water vascular system of the worms, and believes that a parentage with these higher forms is thus indicated. Gegenbaur, moreover, expresses himself strongly against the unicellular theory. He regards, however, the absence of distinct cell nuclei in the substance of the Infusoria as affording evidence of their composition out of several "Cytodes" or non-nucleated protoplasm masses rather than out of true nucleated cells.

* Claparede et Lachmann, "Etudes sur les Infusoires et les Rhizopodes." Genève, 1858-61.
† Greeff, "Untersuchungen über den Ban und die Naturgeschichte der Vorticellen." Archiv für Naturg., 1870.
‡ "Grundsüge der Vergleichenden Anatomie," 1870.

Still more recently Bütschli has given us the results of observations on the conjugation of *Paramœcium aurelia.** He is led, however, to doubt the validity of the sexual interpretation of the conjugation. He found that in certain cases in *Paramœcium aurelia* and in *P. colpoda* the so-called spermatic capsule into which the nucleolus had become converted, had entirely disappeared without any evident change in the nucleus; and he concludes that fecundation of the bodies regarded by Balbiani as eggs cannot be here entertained. Indeed, he will not allow that we have evidence entitling us to regard the appearance of filaments in the interior of the nucleolus as affording any indication of true spermatozoa. He offers no explanation of this appearance, but he calls attention to the fact that both Balbiani and Stein noticed that in *transverse* division of the Infusoria—a phenomenon with which conjugation can have nothing to do—the nucleolus frequently enlarges and acquires a longitudinal striation like that of the nucleolus in the supposed production of spermatozoa during conjugation. Balbiani maintains that this striation during cleavage is only superficial, but it nevertheless affords an argument against assigning any more important significance to the very similar appearance in the case of conjugation.

On the whole it would appear that the spermatozoal nature of the striæ visible in the nucleolus of the conjugating individuals—even admitting that these striæ represent isolatable filaments—has not by any means been proved, while the phenomenon of conjugation in the Infusoria would seem to correspond rather with the conjugation so well known in many lower organisms, where it takes place without being in any way connected with the formation of true sexual products.

In the same memoir the results of observations on some other points in the structure and economy of the Infusoria have also been given by Bütschli. He records the occurrence of minute crystal-like laminæ in the interior of a marine Infusorium (*Strombidium sulcatum*) rendered remarkable by a conspicuous girdle of trichocysts which surround its body. The crystal-like corpuscles seem to be of the nature of starch, for on the application of iodine they assume a beautiful violet colour. It does not appear from Bütschli's account of these bodies that they have not been introduced from without, and the chief interest of the observation seems to be in the discovery of an amyllaceous body assuming a crystalline form. He had previously met with similar bodies in a parasitic Infusorium (*Nyctotherus ovalis*), as well as in a Gregarina (*G. blattarum*).

He also describes, under the name of *Polykricos Swartzii*, a new Infusorium which he frequently found in the fjords of the south coast of Norway and in the Gulf of Kiel, and which he regards as especially interesting, from the fact that with a true infusorial organisation it contains, irregularly distributed in the outer layer of the body, numerous capsules indistinguishable from the true cœlenterate thread cells. These bodies, however, are never included in a special investment, and he justly regards their presence as affording no argument against the unicellular nature of the Infusoria. He lays it down as a probable distinction between the trichocysts of the Infusoria and genuine thread-cells, that the former have the power of ejecting their contained filament from both ends of the capsule, while we know that in the thread cell it is only one end which gives exit to it. This double emission of a filament appears to have been observed by Bütschli in the trichocysts of a large Nassula, but the distinction is certainly not a generally valid one. There is no doubt that in the majority of cases the trichocyst emits its filament from only one end of its capsule, exactly as in the thread cells of the Cœlenterata, and it is hard to see in what respect the bodies noticed by Bütschli in his *Polykricos Swartzii* essentially differ from true infusorial trichocysts. In conlusion, he declares himself strongly in favour of the unicellularity of the Infusoria.

(*To be continued.*)

* O. Bütschli, "Einiges über Infusorien." Archiv f. Microscop. Anat., 1873.

SCIENTIFIC SERIALS

THE current number of the *Journal of Anatomy and Physiology* contains much valuable scientific work, together with its excellent Reports on the progress of Anatomy and Physiology, by Prof. Turner and Dr. Stirling. The first paper is one on the freezing process for section-cutting, and on various methods of staining and mounting sections, by Mr. Lawson Tait. The author prefers the non-employment of chromic acid, picric acid, and other chemically-interfering agents. His section-cutter is a modification of Stirling's, a freezing tank of considerable size being added. The air-bubbles are removed from the sections by the action of boiled water. Logwood and litmus are preferred as staining agents, and their operation is given in proof of the nuclei of cells being, contrary to ordinary ideas, alkaline.—Prof. Flower, in a note on the construction and and arrangement of anatomical museums, makes several very valuable suggestions, which should be specially studied by those who have anything to do with the establishment and construction of biological museums. He shows how that in lofty rooms, with galleries, lighted at the ceiling-wall junction, the preparations have to be arranged according to their manner of preservation; dry, in bottles, and otherwise; which involves the separation of those illustrating any single subject. A series of small side-lighted rooms allows of all the specimens illustrating any single subject, however preserved, being placed in juxtaposition, whilst it separates off the subjects.—Dr. Hollis remarks, with several interesting historical references, on lopsided generation.—The next paper is by Mr. Walter Pye, entitled observations on the development and structure of the kidney. The relation of the capsule to the Malpighian tuft is explained upon the peculiarities observed in the developing organ in a manner differing from the results of Riedel. The characters of the ascending limbs of Henle's loops are described in detail. A plate accompanies the paper.—Mr. Lowne, in a note on the mechanical work of respiration, desires to prove that the amount of work performed in the respiratory act is much less than is usually stated, from calculations based on the relation between the velocity of moving gases and the pressure producing motion.—Dr. Howden describes a case of atrophy of the right hemisphere of the cerebrum attended with the same condition of the left side of the cerebellum and the left side of the body, in a woman aged 30.—Prof. Turner figures and describes the Spiny Shark (*Echinorhinus spinosus*) from a specimen captured near Bass Rock, six-and-a-half feet long. The ureters were found to open into the cloaca by a single orifice. There was no cement gland in the oviduct, from which it is evident that the ova have no horny case. The stomach is succeeded by a pyloric tube; pyloric cæca are absent. In comparing *Læmargus* and *Echinorhinus*, which are supposed to be closely related, it is found that the former possesses two large duodenal cæca and no oviducts, whilst in the latter cæca are absent and oviducts developed. Prof. Turner also proves, from a specimen caught off the mouth of the Frith of Forth, that the Postbeagle Shark (*Lamna cornubica*) possesses a spiracle, contrary to the opinion of most authorities.—Mr. D. J. Cunningham gives notes on the Great Splanchnic Ganglion. In twenty-six cases, he failed to detect its presence in six; it is situated on the body of the twelfth dorsal vertebra; it is variable in shape and size. The same author describes a case of lateral curvature of the spine in connection with hypertrophy of the sympathetic nervous system in the lumbar and sacral regions.—Mr. Dwight makes remarks on the position of the femur and on its so-called "true neck."—Drs. Kronecker and Stirling describe in detail experiments on the characteristic sign of cardiac muscular movement. The fundamental fact on which the investigation is based is the law of Bowditch, that "the induction current of the weakest strength which produces a contraction of the heart does not produce the weakest of possible contractions." The fact that after a pulsation has been developed in the heart of a frog, by a certain stimulation, the organ can be made to continue its beating with a diminished stimulus, is compared to the difference between the effort first required to sound a big bell and that necessary to maintain it ringing. The effect of temperature on the cardiac irritability is shown, the heart reaching its maximum mobility at 25° C. After the discussion of the difficult phenomenon of cardiac tetanus, the authors prove that "the cardiac muscles can only act equally with the help of continually new nutrient fluid." The paper is deserving of the attention of all physiologists.—Dr. Kronecker also describes a new digestion-oven with a diffusion apparatus.—Mr. J. C. Ewart has a note on a large organised cyst in the subdural space.—Mr. J. Reoch writes on the decomposition of urea, adducing evidence to show that in urine the urea is changed into carbonate of ammonia by the action of a fungus the germs of which are contained in the atmosphere.—Mr. M. Simpson describes the existence of two precaval veins in a dog, a condition constant in the kangaroo and some other animals.

Report of the Rugby School Natural History Society for the Year 1874.—We are glad to be able to say that this Report is a satisfactory one; all the sections have done a fair quantity of good work, and a large proportion of the papers read has been the work of actual members or associates. The papers are all highly creditable to the authors, and many of them give evidence of well-trained powers of observation. Mr. J. M. Wilson contributes three interesting papers. One, "On the construction of a geological model of the neighbourhood of Rugby," contains some queries and suggestions as to how such a work should be gone about, and we are glad to see that the model has actually been commenced and has already made considerable progress. This is really most profitable work on which to employ the members of the Society. Other papers by Mr. Wilson are, "On the companion of Sirius," a note of an observation on the *comes* of Sirius, from which Mr. Wilson infers that it has performed twenty-three degrees of its revolution in ten years; and "On the Geology of Hillmorton." The following titles of papers by members will give an idea of the work done by the Society:—"On Mounting for the Microscope," by E. J. Power; "On the Will-o'-the-Wisp," by H. W. Trott; "On Owls," by H. Vicars; "On the Sub-Wealden Explorations," by R. D. Oldham; "On an Entomological Expedition," by H. F. Wilson, who also contributes a paper "On the Great Spotted Woodpecker;" "On Migrations," by W. C. Marshall; "On Bees," by H. Vicars; "On Roman Remains near Church Lawford," by L. Knowles; "On Drops of Liquid," by H. F. Newall, a very interesting paper, giving evidence of some faculty for original research; "On Cuckoos," by W. Larden. Mr. Newall's paper on drops is illustrated by some carefully executed drawings. The same member has constructed an ingenious compound pendulum machine, an illustration of which is given, as also illustrations of some most delicate curves executed by the machine. Among other illustrations we may mention a heliotype copy of a drawing by J. H. Patry of fifteen various observations of the planet Mars, taken at the Temple Observatory. Very full sectional reports are appended, and under the head of "Statistics" a variety of information is given. Altogether this is one of the most satisfactory reports published by this Society.

Zeitschrift der Oesterreichischen Gesellschaft für Meteorologie, April 15.—This number contains an article by Prof. Buys-Ballot on the climate of Buenos Ayres, and another on the hailstones which have occurred in Würtemberg during the forty-six years 1828–73.

Bulletin de l'Académie Royale des Sciences de Belgique, 2 ser. t. xxxix. No. 3.—This number contains a note on *Pecopteris odontopteroides* (Morris), by M. François Crépin. There is a fossil from the coal measures of Hobart Town among those sent by Mr. Allport to the museum, which M. Crépin refers to the same species as that named by Prof. Morris. He doubts, however, whether Prof. Morris has assigned his specimen to its true relationships, believes it is nearer to *Odontopteris alpina* (Gein), and provisionally proposes *Odontopteris Morisii* as its name.—On the *calcaire carbonifère* between Tournai and the environs of Namur, by M. E. Dupont; a description of forty-seven pages, with two coloured folding plates of sections.—Researches on the structure of the corda dorsalis of Amphioxus, by M. Camille Moreau. The work was carried on in the microscopical laboratory of the University of Liége, under the direction of Prof. E. Van Beneden. The paper consists of a description with a plate. To complete the working out of the homologues of the layers, further embryological observations, M. Moreau says, are necessary.—No. 4. The communications in this number are:—Note on the temperature of the winter of 1874–75, by M. Quetelet. The winter is compared with that of 1859–60, and a table showing the resemblance is given.—Note on the halo with mock moons of March 23, 1875, by M. Chas. Hooreman.—On the theory of the use of hot air in furnaces, by M. H. Valérius.—On some fossil plants from the "Psammites du Condroz," by M. A. Gilkinet. This paper is partly of criticism on the work of M. Crépin, and is partly descriptive. Three folding plates of illustrations are given.

Archives des Sciences Physiques et Naturelles, vol. 52, No. 207 (March 15, 1875).—This part contains many papers trans-

lated and reprinted from other serials, besides several original ones. We note the following:—On the fossil vertebrata of the State of Nebraska, by M. Delafontaine. On the measurement of altitudes in Switzerland, executed by MM. Hirsch and Plantamour. On the action of galvanic currents upon alloys or amalgama, by M. Eugène Obach. On some experiments with Holtz's machine, by F. Rossetti. Researches on the spectrum of chlorophyll, by J. Chautard.

SOCIETIES AND ACADEMIES

LONDON

Linnean Society, June 3.—Dr. G. J. Allman, F.R.S., president, in the chair.—The President nominated the following gentlemen as Vice-presidents for the ensuing year, viz.:—Mr. G. Bentham, F.R.S.; Mr. G. Busk, F.R.S.; Dr. J. G. Jeffreys, F.R.S.; and Dr. J. D. Hooker, P.R.S.—Prof. Thiselton Dyer exhibited, under the microscope, some specimens of the very rare Alga *Stephanosphæra pluvialis*, known to occur only in a single locality in Britain, a pool on Bray Head, in Ireland.—Dr. Trimen exhibited specimens of two recent additions to the British flora, *Zannichellia polycarpa*, found by Dr. Boswell-Syme in the Orkney Islands in 1847, and *Carex ornithopoda*, discovered by two working men in Derbyshire.—Mr. Pascoe exhibited a very fine collection of Crustacea from the Bay of Naples. The following papers were then read:—On the Barringtoniaceæ, by J. Miers, F.R.S. The purpose of this paper is to show that the Barringtoniaceæ constitute a distinct order, forming an extremely natural group with peculiar and uniform characters, differing from the Myrtaceæ in their alternate leaves without pellucid dots, and in the nature of their inflorescence and fruit. They are trees, frequently of large size, rarely low shrubs, all delighting in running streams, some growing in estuaries or along the sea-shore. The author describes the characters of the order in considerable detail, and gives the diagnosis—in many cases redrawn from actual examination—of each genus and species. The number of genera he makes to be ten. The paper was accompanied with drawings illustrating the floral and carpological characters of each genus.—Note on the occurrence of fairy rings, by Dr. J. H. Gilbert, F.R.S. This paper was founded on the observations made by the author and Mr. Lawes on their experimental plots at Rothamstead. After some particulars as to the effect of different manures in varying the proportion of different kinds of vegetation in permanent pasture, especially grasses and Leguminosæ, the author suggests that the determination of the source of the nitrogen in the fungi that constitute the fairy rings which frequently make their appearance on the plots would throw some light on the much-disputed question of the source of the nitrogen of the Leguminosæ. It is remarkable that although, according to published analyses of various fungi, from one-fourth to one-third of their dry substance consists of albuminoids or nitrogenous matter, and 8 to 10 per cent. of mineral matters or ash, of which about 80 per cent. is potassium phosphate; yet the fungi develop into "fairy rings" only on the plots poorest in nitrogen and poorest in potash. The questions which appear still to require solution are these:—(1) Is the greater prevalence of fungi under such circumstances due to the manurial conditions themselves being directly favourable to their growth? or (2) Are the lower orders of plants—in consequence of other plants and especially grasses growing so sluggishly under such conditions—better able to overcome the competition and to assert themselves? (3) Do the fungi prevail simply in virtue of the absence of adverse and vigorous competition, or to a greater or less extent as parasites, and so at the expense of the sluggish underground growth of the plants in association with them? or (4) Have these plants the power of assimilating nitrogen in some form from the atmosphere; or in some form or condition of distribution within the soil, not available, at least when in competition, to the plants growing in association with them?—On a possibly wild form of *Hibiscus Rosa-sinensis*, by Prof. Oliver, F.R.S.

Mathematical Society, June 10.—Prof. H. J. S. Smith, F.R.S., president, in the chair.—Prof. Cayley, F.R.S., made a brief communication on some figures of curves in 3-bar motion.—Prof. Sylvester, F.R.S., spoke on "James Watt's parallel motion," and on an apparatus for regulating the motion of a train of prisms.—Mr. T. Cotterill read a paper on the correspondence of points collinear with a fixed origin. In the paper S and T are taken homogeneous functions of any number of variables (say three, $x\ y\ z$): the degree of S being one lower than that of T, and are supposed to be connected with another set, $x'\ y'\ z'$, of the same number of variables by the equations $\frac{x'}{x} = \frac{y'}{y} = \frac{z' S}{T}$. If the variables $x\ y\ z$, $x'\ y'\ z'$, denote the co-ordinates of two points in a plane, a correspondence is established between them depending on the forms of S and T. The object of the paper is to explain the relations between the corresponding curves and to give examples.

Physical Society, June 12.—Prof. Gladstone, F.R.S., president, in the chair.—Lord Lindsay, Sir W. Thomson, and Prof. Sylvester were elected members.—Mr. Wildman Whitehouse described some experiments he had made on the electric conductivity of glass. He employed pieces of thermometer tube about an inch in length, into the bore of which two platinum wires were inserted in such a manner that there was an interval between the points. In some cases one wire of platinum occupied the entire bore of the tube, and this tube was surrounded on its external surface by a helix of wire of the same metal. In each case the arrangement was introduced into a circuit in which were also placed a Thomson galvanometer and a set of resistance coils. It was shown that at the ordinary temperature there was no deflection, but that the current passed freely when the glass was heated to redness. The difficulty of making contact with the glass led Mr. Whitehouse to use two test-tubes, one inside the other, both containing mercury, with which wires of platinum freely communicated. The flame of a Bunsen burner was applied to the outer test-tube and the temperature of the metal noted by the aid of a thermometer. In one series of experiments the diameter of the internal tube was $\frac{5}{8}$ inch, the length in contact with the mercury about $3\frac{3}{4}$ inches, and the thickness of the glass $\frac{1}{100}$th of an inch. A current was first observed to pass at 100° C., and, as the temperature rose, the amount of deflection increased. The following are approximate measurements of the resistance of the glass at different temperatures:—

At 165° C.	Resistance	=	229,500	Ohmads
,, 185 ,,	,,	=	100,000	,,
,, 210 ,,	,,	=	69,000	,,
,, 255 ,,	,,	=	22,500	,,
,, 270 ,,	,,	=	9,000	,,
,, 300 ,,	,,	=	6,800	,,

Prof. Gladstone drew attention to the necessity for ascertaining the nature and composition of the glass.—Prof. Guthrie alluded to the fact that electricity of high tension is freely conducted by glass at a red heat. He also asked whether, as the temperature was raised, a point was reached at which the conductivity began to decrease.—Prof. M'Leod pointed out that the thermometer tubes used by Mr. Whitehouse were of lead glass, and that the lead had in most cases been reduced by exposure to the flame of the Bunsen burner, and he urged that these facts should not be overlooked in measuring the resistances. He stated that lead glass is better than other kinds of glass for insulation.—Prof. G. C. Foster asked whether an increased capacity due to the heating might not introduce an error into the measurements of resistance. Mr. Whitehouse replied that he had only recently commenced the experiments, and promised that the suggestions which had been made should receive due attention.—The President then read a paper on the time required for double decomposition of salts. It is well known that if, on mixing solutions of two salts, MR and $M'R'$, an insoluble body can be produced by an interchange of metals and radicals, that body is produced to the fullest extent possible. The only explanation of this fact which has been given is founded on the theory of Berthollet, that in all cases of mixture there is a redistribution of the constituents according to their relative affinity and mass, with the production of more or less MR' and $M'R$. Now, if one of these, say MR', be insoluble, it will remove itself at once from the sphere of action, but this will necessitate a fresh distribution of the constituents with the production of more insoluble salt, and so on until the whole of the M has entered into combination with R'. Dr. Gladstone commenced this research twenty years ago, and added in a note to a paper in the Phil. Trans.: "It is easily conceivable that when the affinity for each other of the two substances that produce the insoluble compound is very weak, the action may last some time and become evident to our senses. Is not this actually the case when sulphate of lime in solution is added to nitrate of strontia, or carbonate of soda to chloride of calcium, or an alkaline carbonate to tartrate of yttria, or oxalate of

ammonia to sulphate of magnesia, &c.?"—The President gave several experimental illustrations of the time required for double decomposition. He showed that ferric chloride and sulphocyanide of potassium react instantly, that citrate of iron and meconic acid, chloride of platinum and iodide of potassium, react gradually. The rate of change really depends on the degree of rapidity of the inter-diffusion of the salts. It is also affected to a very great extent by temperature. The following numbers illustrate the rate at which sulphate of strontium is deposited on the addition of sulphate of calcium to a solution of nitrate of strontium.:—

Cloud		in	4 minutes
0·071	grms.	,,	20 ,,
0·130	,,	,,	60 ,,
0·303	,,	,,	110 ,,
0·497	,,	,,	170 ,,
0·659	,,	,,	1270 ,,

The total amount of salt which could be formed being 1·5 grms.

Astronomical Society, June 11.—Prof. Adams, president, in the chair.—Mr. Lecky explained the use of two ancient instruments he had given to the Society. The smaller one was known as a night dial; it was used about the end of the sixteenth century for finding the time at night by the position of the pointers of the Great Bear. The observer stood with his face to the north, and the instrument was held in one hand, so that a line upon it was by estimation vertical to the horizon; and with the other a moveable arm like a clock hand was turned until it was parallel to the direction of the pointers. The time was then read upon the circumference of a boxwood circle, which had to be set afresh for every night of the year. The other instrument was a Backstaff, which was used at sea until the invention of the sextant for determining the sun's altitude. The observer in using it stood with his back to the sun (whence its name), and he measured the arc between the sun's place and the opposite horizon through the zenith. The instrument which was in use before this was a very simple contrivance, being merely a pole along which a moveable bar at right-angles was shifted, until the cross-bar subtended the same angle when looked at by the observer with his eye at the end of the pole as the sun's altitude. Such contrivances were called Forestaffs, and were in use at sea until 1591, when Capt. Davis invented the Backstaff.—Mr. Marsh exhibited a drawing of the orbits of the satellites of Saturn as they will be seen from the earth about the middle of August next, when there will be a conjunction of the satellite Iapitus with the ring and ball of Saturn. Mr. Marsh was anxious that observations of this conjunction should be made by the possessors of large telescopes, in order to afford data for the improvement of the theory of the satellites of Saturn.—A paper was read by Mr. Knobel on an instrument for determining the magnitudes of stars.—Mr. Christie said that the probable error in determining the magnitude of a star with his photometer amounted to only the twentieth of a magnitude, but that the probable error varied for stars of different colours, owing to the effect of contrast with the light of the sky, which caused a red star to be more easily distinguished when its light was diminished than a star with a blue tinge.

Anthropological Institute, June 8.—Col. A. Lane-Fox, president, in the chair.—Capt. Richard F. Burton, H.M. Consul at Trieste, read two papers on Ancient Remains in Dalmatia, viz., "The Long Wall of Salona" and "The ruined cities of Pharia and Gelsa di Lesina." Salona was the Roman metropolis of Dalmatia, of which southernmost province of Austria, Spalato was at present the natural, and Zara the artificial and political capital. The "long wall" was of doubtful and debated origin, and a reference to numerous ancient and a few modern writers on it was made to show the obscurity in which it still remains. The author gave an account of his explorations, with detailed measurements of the ancient structure, called by some "Cyclopean," and especially pointed out the great variety of stone dressing it presented, which would afford valuable evidence in determining the style and perhaps the date of the work. His conviction that the long wall of Salona was Greek and pre-Roman rested very much upon the fact that similar constructions exist in the neighbourhood. In the island of Lesina the two ruins visited and described by Capt. Burton presented a remarkable resemblance, amounting almost to identity, to the long wall of Salona, and suggested that they were all the work of a single people, and that people not the barbarous Illyrians, but the comparatively civilised Greeks. Only two flint implements had been found, and those were discovered at Salona, near Spalato. The exploration of the Dalmatian Islands was attended with much difficulty; the scarcity of water was an evil to be met, and a Slavic guide was necessary unless the traveller could himself speak Slavic, for the inhabitants all belong to that race. The islands never having been previously explored (as far as the author was aware) by Englishmen, there was a large field of research for the antiquarian as well as the more general anthropologist.

PARIS

Academy of Sciences, June 7.—M. Frémy in the chair.—The following papers were read:—On the different effects produced by the same temperature upon the same species of plants, in the north and in the south, by M. A. de Candolle.—Researches on magnetic rotatory polarisation, by M. Henri Becquerel.—On a new method and a new instrument for telemetry (quick measurement of distances), by M. Giraud Teulon.—On the transformation of the camphor of *Laurineæ* into camphene, and reciprocally of the camphenes into camphor, by M. J. Riban.—A note, by M. J. Ponomareff, on thiammeline, a new derivative of persulphocyanogen.—On the dissociation of sulphocarbonate of potassium in the presence of ammonia salts, by M. Rommier.—On the theory of revolution surfaces which, by way of deformation, can be superposed on one another, and each on itself in all its parts, by M. F. Reech.—Communications on Phylloxera, by several gentlemen.—Several papers of minor interest, competing for the prize of Medicine and Surgery.—On the geographical position of the island of St. Paul, by M. Mouchez; he finds the latitude to be 38° 42′ 50″·796 S. (with a probable error of 0″·03), and the longitude, 5h. 0m. 49s. (probable error, 4s.).—On fluorene and the alcohol derived from the same, by M. Ph. Barbier.—Researches on taurine, by M. R. Engel.—On the bibromide of angelic acid, by M. E. Demarcay.—On three observations of accidents from lightning, by M. Passot.—Analysis of the mineral coal of the Suderoe Island (one of the Faroes), by MM. Bechin and Ch. Mène.—Remarks by M. Tresca, on a projected atmospheric post between Paris and Versailles.—A note by M. Emm. Liais, on the parallax of the sun.—M. Vibraye then drew the Academy's attention to the apparition of a destructive hemipterous insect in the vineyards of the Loir et Cher Department. The insect is very similar to *Phytocoris gothicus*.—Remarks by M. J. de Cossigny, on waterspouts.—On a new propeller of steamships, by M. E. Lehman.

BOOKS AND PAMPHLETS RECEIVED

BRITISH.—Encyclopædia Britannica, 9th edit., vol. ii. (A. and C. Black).—On the Principles and Management of the Marine Aquarium: Wm. R. Hughes, F.L.S. (John Van Voorst).—The Life and Growth of Language. International Series: W. Dwight Whitney (Henry S. King and Co.)—First Annual Report of the Yorkshire College of Science, Leeds.—The Positive Philosophy of Auguste Comte: freely translated and condensed by Harriet Martineau. 2 vols. (Trübner)—The Geological Evidences of the Antiquity of Man reconsidered—an Essay by Thos. Karr Callard, F.G.S. (Elliot Stock).—Corals and Coral Islands: Jas. D. Dana (Sampson Low and Co.)—An Introduction to the use of the Mouth-Blowpipe: Scheerer and Blandford (Frederic Norgate).

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